THE JOURNAL
OF THE
Society of Chemical Industry:
A MONTHLY RECORD
FOR ALL INTERESTED IN CHEMICAL MANUFACTURES.

No. 3.—Vol. IV. MARCH 30, 1885. Non-Members 30/- per annum; Members 21/- per Set; Single Copies 2/6.

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The Executive Council of the Inventions Exhibition, having on the motion of Sir F. A. Abel, C.B., D.C.L., F.R.S., and Chairman of the Juries Commission, kindly allowed to members of the Society the privilege of season tickets at half-price, these may be had upon application, accompanied by remittance of 10s. 6d., to the General Secretary at the Society's Office, Palace Chambers, Bridge St., Westminster, S.W.

Members are hereby informed that the Annual General Meeting (1885) will take place in London in or about the second week in July. Details and arrangements will be published in due course, and members will receive their tickets of membership, which, as heretofore, will form vouchers in visiting works and places of interest.

Authors of papers printed in the Journal are hereby notified of the fact that, in accordance with Bye-law 43, they are entitled to receive not more than 50 gratuitous copies of their papers. Authors should state on their manuscripts their desire to have free copies, adding the number wished for. Unless the contrary be specially desired, this being stated on the manuscript, the reprints of an author's paper will not include the report of any discussion that may have arisen after the reading of the paper.

Members are hereby advised that the Subscription for 1885 falls due at the beginning of the year, and should be paid as soon afterwards as possible. The Council has decided that after January 1st, 1885, the 29th Bye-law will be strictly enforced, so that Members whose subscriptions are more than four months in arrear, will not continue to receive the Society's Journal.

Inquiries having been made as to the transactions of the Newcastle Chemical Society, the Committee of the Newcastle Section have to state that a very few complete sets are remaining, and will be sold to members at £2 10s. per set. A number of separate volumes can also be had by members wishing to complete their sets, at 10s. per volume (generally three years' issue). Application for these should be made to the Local Secretary of the Newcastle Section.

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MEETINGS, SESSION 1885—First Monday in each month (unless otherwise indicated).

April 13.—(First Monday *after* Easter)—Mr. W. J. Dibdin—"Further Notes on the 'Radial' Photometer, and the Proposed Standards of Light."

May 4.—Messrs. Giles and Shearer—"The Real Strength of Aqueous Solutions of Sulphurous Acid."

Messrs. C. Napier, Hake, and Mook—"The Stassfurt Salts Industry, and its bearing on other Industries."

Discussion on Professor Munro's paper published in January number of the Journal, "On the Manurial Value of Filter-Pressed Sewage Sludge," read January 5.

June 1.—Messrs. Salamon and W. de Vere Mathew—"The Influence of Phosphates upon Fermenting Worts."

July.—The Annual General Meeting.

Notices of papers and communications for the meetings to be sent to the Local Secretary.

Meeting held March 2, 1885.

SOME EXPERIMENTS UPON THAT PART OF THE PATENT OF MR. F. B. RAWES FOR THE RECOVERY OF SULPHUR, ETC., WHICH DEPENDS UPON THE ACTION OF CARBON-DIOXIDE UPON SODA WASTE OR SULPHIDE SUSPENDED IN A LIQUID.

BY WM. JOEL KEMP.

IN the process in question the reliance is upon carbon-dioxide as the chemical agent for the elimination of hydrogen-sulphide, the carbon-dioxide being applied in the manner detailed in the patent taken out by Mr. Rawes early in the year 1882. It appeared to me that the suspension of the sulphide in a liquid "intermedium," and the systematic agitation specified, were definite, and most important advances upon all previous attempts in the same direction, and therefore the process seemed to claim an especial interest.

My experiments now to be described were carried out during August and September, 1882, and the proposition started with was:—

"Given pure carbon-dioxide to operate with, whether hydrogen sulphide would be produced sufficiently free from admixture with carbon-dioxide to be utilisable as a source of sulphur or sulphuric acid; and whether practically the whole of the sulphur existing as sulphide could be eliminated by the method."

The waste used was partially oxidised, and contained a total of 21·44 parts of sulphur in 100 parts of dried waste, three-fifths of the sulphur being convertible into hydrogen sulphide by treatment with hydrochloric acid.

In making the first experiment a measure of moist waste equal to about 20cc. was divided into three approximately equal parts, and each part placed in a wide-mouthed bottle with some bi-carbonate of soda solution as an "intermedium," the use of an "intermedium" being part of the process alluded to. Each of the three bottles (Fig. 1) was fitted with a cork pierced with three holes—a larger one in the centre through which passed a glass tube with its lower end dipping into the liquid, and reaching at the upper end to about eight inches above the rim of the bottle. This tube was sufficiently wide to admit of the easy working of a stout wire rod which passed through it, and which was bent at the lower end in such a manner as to form a stirrer.

Through the smaller holes glass tubes were passed in such a way as to represent the usual arrangement of the Woulfe's bottle. The bottles were then connected in series, and the first of the series attached to a Kipp gas generating apparatus charged with marble and hydrochloric acid, and provided with a wash bottle containing water.

The exit tube of the third bottle was connected with two flasks coupled together in series, charged with copper sulphate solution slightly acidulated with sulphuric acid. Next the second flask was a drying bottle containing concentrated sulphuric acid, and then a small weighed flask with caustic potash to retain any CO_2 that might pass over unabsorbed.

The upper end of each stirrer worked in a bearing, and was fitted with a pulley. The pulleys were coupled together by elastic bands, and one of them attached by the same means, to a small Griscombe (electric) motor. The three bottles, simulating the "desulphurers" described in the patent, were placed in a water bath, provided with a source of heat, the complete arrangement being shown in the accompanying illustration (Fig. 2).

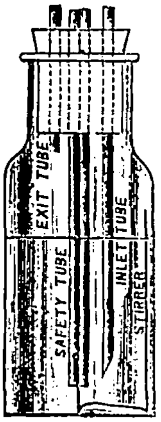

FIG. 1.

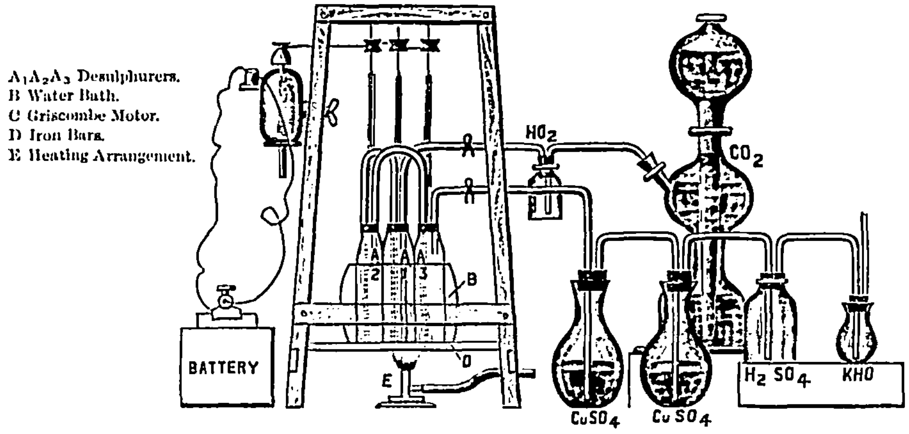

FIG. 2.

The motor was started and a slow stream of CO_2 admitted, and after some time the copper sulphate solution darkened in colour, and upon heating the water bath to about 45° C. a decided precipitate appeared. The pressure of carbon-dioxide was only sufficient to propel a very weak current of gas through the apparatus, and the stirrers worked only slowly, in consequence of the pullies requiring readjustment. After five hours the experiment was stopped, the receivers disconnected from the desulphuring bottles, and a current of air aspirated through the former.

The copper sulphide was then collected on a filter, dissolved, and precipitated with caustic potash, and the copper weighed as oxide. The potash bottle was also weighed, and from the results the respective weights of hydrogen sulphide and carbon-dioxide that were absorbed were calculated as follows:—

H_2S = ·081grams = 96·43 per cent.
CO_2 = ·003 „ = 3·57 „

These results appeared satisfactory as to the percentage composition of the resulting gases, but the small amount of gas expelled determined me in continuing the experiments to provide for a greater pressure of carbon-dioxide, which was accomplished by fitting a tube by a cork into the neck of the Kipp apparatus, to the upper end of which tube was fixed by a pierced cork a cylinder open at the top to serve as a reservoir.

The working of the pullies and stirrers was very much improved, so that in the later experiments it was easy with a current from three Groves' cells to give them a very rapid rotation.

The receiving arrangement was also remodelled as shown in Fig. 3.

The exit tube A from the last desulphurer of the series was bent at an obtuse angle at B, and was kept cool with moistened filtering paper during the experiments to condense any slight amount of moisture. C and D were charged with a known volume of a standard solution of copper sulphate, and were placed in a water bath kept at a temperature of about 40° C. E contained a small quantity of lead nitrate to indicate if any hydrogen sulphide passed the copper sulphate unabsorbed. F was a drying bottle with concentrated sulphuric acid, G with caustic potash to take up the carbon-dioxide, and in the test tube H was a small quantity of concentrated sulphuric acid to prevent the entrance of carbon-dioxide and moisture from the air.

The receivers C and D were in duplicate, and the duplicates were used alternately in order that it might be possible to wash and dry one set before recharging with copper sulphate solution, during which time the other set could be used. This arrangement was necessary to render it possible to estimate the amounts of the gases evolved at various stages of the operation. The method adopted to estimate the copper sulphide precipitated was as follows:—

After each stoppage the receivers were disconnected from the desulphurers at I, and the exit tube of the latter was plugged. Air was then aspirated through the receiving apparatus C to H, after which the contents of the tube D were added to those of the flask, and the whole shaken up together, and a small portion of the solution quickly filtered through a dry filter. 10cc. were then taken by a pipette from the filtrate, and the copper, after reduction by sulphurous acid, was precipitated by an excess of ammonium thiocyanate. The precipitate being filtered off the excess of ammonium thiocyanate was estimated by titration with silver nitrate, and the hydrogen sulphide calculated from the results.

In carrying out this method for the estimation of the residual copper, some slight oxidation of the copper sulphide may have occurred during the aspiration, and the filtration of the copper solution; but the error, if any, due to this cause must have been until the issuing gas almost ceased to precipitate any sulphide from the copper solution.

The liquid was then poured off and a portion of the residue, after being washed and dried at 100° C., was distilled with hydrochloric acid, the distillate being received into a solution of copper acetate, which proved that the solid residue was practically insignificant. In the second experiment (*i.e.*, the first under the new arrangement) two desulphuring bottles were employed instead of three, as before; and a larger quantity of waste was operated upon.

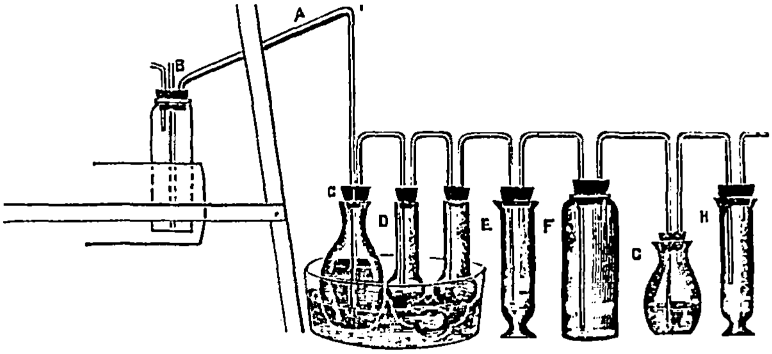

FIG. 3.

A measure (about 20cc.) of the moist waste was placed in each bottle, the bicarbonate of soda solution added, the stirrers rotated, and a current of carbonic anhydride passed in. The results at stated intervals are given in the following table :—

Number of Stoppages.	Number of Hours run.	Weight in grammes of Gases collected.		Percentage composition of Gases collected.	
		H_2S	CO_2	H_2S	CO_2
1	2	·2067	·0217	90·93	9·07
2	1½	·4371	·0200	95·62	4·38
3	1	·3117	·0900	77·76	22·24
4	1	·4530	·1250	78·37	21·63
5	2	·2200	·0200	91·66	8·34
6	3	·1695	·0170	90·88	9·12
7	1	·2968	·0100	88·12	11·88
8	3	·6350	·0700	90·07	9·93
9	2½	·1130	·0100	91·17	8·83
	17	3·1158	·4137	87·63	12·37

The above table shows some remarkable differences in the composition of the escaping gases—for instance, see Nos. 3 and 4, where the percentages of hydrogen sulphide were only 77·66 and 78·37 respectively.

In these cases the battery was weak, and the agitation consequently insufficient to utilise fully the quantity of carbon-dioxide being passed in. This point will be referred to later on. Whilst Nos. 5 and 6, where the gas expelled is small in amount, show that the pressure of carbon-dioxide was weak owing to the need of replenishing the supply of acid in the generating apparatus.

As the residue from this experiment was not exhausted of sulphide, it was further subjected to treatment with carbon-dioxide for nine hours, and exhausted of sulphide, as no precipitate of copper sulphide was *formed—a mere trace being indicated* by a slight brown colouration at the edge of the copper solution. And this trace was probably due to iron-combined sulphur existing originally in the waste, which would of course not be acted upon by carbon-dioxide.

The next trial was conducted with one desulphuring bottle *only*, the receiving arrangement remaining as before.

Two measures of moist waste (each equal to about 20cc.) were acted upon with results stated in the following table :—

Number of Stoppages.	Number of Hours run.	Weight in grammes of Gases collected.		Percentage composition of Gases collected.	
		H_2S	CO_2	H_2S	CO_2
1	1	·1713	·029	83·25	16·75
2	1½	·2130	·074	76·65	23·35
3	1	·2762	·121	70·00	30·00
4	1	·2700	·213	56·25	43·75
5	2	·5038	·228	68·85	31·15
6	2½	·6271	·185	77·22	22·78
7	1½	·5712	·188	74·80	25·20
8	2	·1965	·307	61·77	38·23
9	3	·3391	·208	62·00	38·00
10	2	·2216	·120	64·80	35·20
	17½	3·7261	1·673	69·01	30·99

From the above table it will be seen that when approximately the same quantity of waste formerly acted upon in two connected desulphuring bottles, was now operated upon in one, a much larger percentage of carbon dioxide passed through, showing that the distance travelled by the gas in its ascension through the suspended waste has a marked effect upon the respective proportions of hydrogen sulphide and carbon dioxide coming off. As might be supposed, the deeper the column of "intermedium" (containing a given quantity of waste) the better the result in hydrogen sulphide.

For this reason, and also because a great absorption of carbon dioxide takes place at first starting, it is evident that an arrangement, as suggested by the patentee, which permitted the use of two or more desulphurers in series, and an interchange in their order, would be most desirable. In such a case, the excess of carbon dioxide passing from the first would be utilised by the fresher waste in the second, third, etc. The propriety is also pointed out of having the desulphuring vessels as deep as circumstances will allow.

The next experiment was carried out with a view to ascertain whether, by greatly increasing the number of points of contact between the ascending gas and the suspended particles of waste, a greater current of carbon dioxide could not be forced in without danger of its passing away unabsorbed; and to this end, one desulphuring bottle was charged with five measures (each about 20cc.) of waste and a bulk of intermedium, reaching to the same height as in the previous experiments.

The results obtained were as follows :—

Number of Stoppages.	Number of Hours run.	Weight in grammes of Gases collected.		Percentage composition of Gases collected.	
		H_2S	CO_2	H_2S	CO_2
1	4	·3721	·0515	87·83	12·17
2	2	·4388	·0510	89·55	10·45
3	1	·2150	·0505	81·13	18·87
4	1¼	·9121	·0430	95·51	4·49
	8¼	1·9383	·1960	90·80	9·20

During the first 3 to 4 hours the absorption of carbonic anhydride was very great, and the quantity of hydrogen sulphide expelled comparatively small, owing to the hydrate of lime contained in the waste, which required saturating by the carbon dioxide before the latter could act to any extent upon the calcium sulphide.

This absorption of carbon dioxide had been anticipated, of course, and noticed in the earlier experiments, but in the present case it was more marked, in consequence of the much larger quantity of waste being operated upon.

At this point another desulphurer, similarly charged to the first, was attached thereto, and the experiment continued, the respective weights and proportions of the gases evolved being annexed :—

Number of Stoppages.	Number of Hours run.	Weight in grammes of Gases collected.		Percentage composition of Gases collected.	
		H_2S	CO_2	H_2S	CO_2
1	1	·7310	·019	97·48	2·52
2	¾	1·2530	·037	97·13	2·87
3	½	·9475	·039	96·05	3·95
	2¼	2·9315	·095	96·85	3·15

From these two last experiments I found, as surmised, that by acting upon a larger quantity of sulphide in a given bulk of "intermedium," it was possible to pass in a larger supply of carbonic anhydride, and consequently to obtain a richer result in hydrogen sulphide, almost free from admixture with carbon dioxide.

It now occurred to me that if, from the small apparatus with which I was working, I could obtain a current of hydrogen sulphide in sufficient quantity to burn from a jet, it would be an attainment not devoid of interest, and to that end I now proceeded to adapt my apparatus as shown in Fig. 4.

Three desulphuring bottles were now charged, each with about the same quantity of waste as in the last experiment, and the gases evolved from the desulphurers were made to pass through three bottles, A, B, and C (all containing solution of normal sodium carbonate), which represent the purifiers designed by the patentee for the absorption of waste carbon dioxide. D, E, and F contained absorbents of hydrogen sulphide, and were used to prevent the escape of the gas, should it not be obtained in sufficient quantity to burn from the jet, as I had no fume closet of sufficient size available. H was filled with dried calcium chloride.

Four cells of a Groves' battery were connected with the motor, which gave the agitators considerable velocity, and a current of carbon dioxide was forced through the apparatus. After a short time, clip K was opened, and, on a light being applied to the platinised burner G, the gas burned freely, with a flame some 2in. to 3in. long.

The experiment was repeated several times with varying degrees of success, and the best results were obtained when the agitation was most efficient.

There was a marked relation between the speed of the agitators and the quantity of carbon dioxide absorbed on the one hand, and of the hydrogen sulphide evolved on the other. As an instance: upon one occasion the agitators were only working at a moderate speed, and under those conditions it was not possible to keep the gas burning on account of the quantity of CO_2 present, my arrangement of normal sodium carbonate solutions not being sufficient to absorb all the carbon dioxide when a strong current of that gas was passing. On putting on, however, an additional cell, which probably doubled the speed of the stirrers, the carbon dioxide was absorbed and the issuing gas burned freely. It is thus evident that the efficiency, or otherwise, of the agitation is sufficient to make all the difference between success and failure.

The short piece of tubing L, surrounding the burner, steadied the flame and prevented its floating away as it was sometimes inclined to do, doubtless through the presence of carbon dioxide. I suppose the piece of outside tubing lessened the dissipation of heat from the burner, so that the gas and air met at a higher temperature than when the tube was dispensed with.

With regard to the application of heat to the desulphurers, it was found that by heating the water bath in which they were to about 45° C., the commencement of the evolution of hydrogen sulphide was accelerated.

In preparing each of the foregoing experiments, the addition of the bicarbonate of soda solution to the waste caused an immediate evolution of hydrogen sulphide, showing, I think, that the view is correct that the bicarbonate acts as a carrier of carbon dioxide to the sulphide.

Remembering that my method of passing in the gas, although as convenient as I could readily adopt, is not to be compared in point of efficiency with arrangements that could be made with apparatus upon a larger scale, and taking this into consideration, together with the results obtained, it must, I think, be confessed that the action of carbon dioxide upon soda waste, under the conditions prescribed by Mr. Hawes, is a question of considerable interest.

Although I have spoken above of the action of carbon dioxide upon calcium sulphide, I have not overlooked the researches of Dr. Divers and others upon the changes which take place in alkali waste, etc., but I have sought simply to give the practical results of my experiments, leaving out of consideration, at the present time, those intermediate steps by which the final results may have been attained.

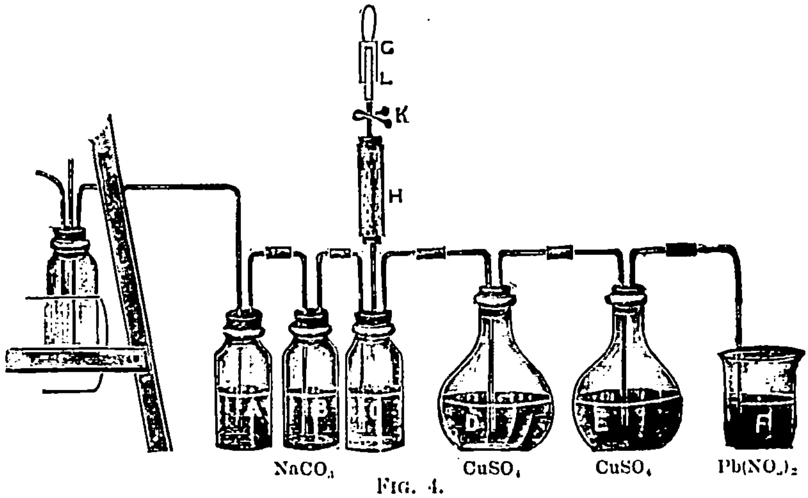

Fig. 4.

The present investigation being very incomplete on certain points, it was intended to have followed it up by other experiments calculated to throw light upon the question as to the best temperature at which to keep the desulphurers, and, again, others to show the differences in composition of both the solid and liquid contents of the bottles, before and after treatment of definite amounts of waste and intermedium, with definite quantities of carbon dioxide, for definite periods of time, etc.

Pressure of work, however, in other directions has, so far, prevented these intentions being carried out.

DISCUSSION.

Mr. Cresswell said, that the weak point in Mr. Kemp's interesting paper consists in his silence as to efficient means for procuring that pure carbonic acid on a commercial scale on which he relies for his reaction. Without such provision the practical aspect of the paper would at once disappear. He might inform the meeting that while working the Schaffner and Helbig process at Messrs. Chance's Works, sulphuretted hydrogen had been made on such a large scale that it had been used in flames sometimes more than a foot long for making chamber acid, and burnt as freely as coal gas. In decomposing waste by limekiln or flue gases this direct burning was impossible, and a supplementary process such as that of Mr. Claus' became necessary.

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THE TREATMENT OF CERTAIN PHOSPHATIC MINERALS OF THE TYPE KNOWN AS "REDONDA" PHOSPHATES.

BY W. J. WILLIAMS.

In 1878 the problem was laid before me of how to deal with the phosphates of alumina and iron known as "Redonda" phosphate, so as to produce therefrom marketable products.

Two methods at once suggest themselves: (*a*) solution in mineral acid, (*b*) furnacing with alkalies or alkaline salts.

In considering the possibilities of solution the solvents are practically reduced to two—viz., hydrochloric and sulphuric acids.

Hydrochloric acid was quickly dismissed, as this acid *dissolves* the phosphate but does not *decompose* it, and on neutralisation of the acid solution, phosphate of alumina and iron is simply re-precipitated. The only advantages derivable from this method of treatment are, first the siliceous matter can be separated by decantation, and second, the phosphate of alumina and iron is re-precipitated in a very finely divided condition.

Treatment with *Sulphuric Acid.* In this method I was unable to devise any improvement on the well-known process of the late Mr. Peter Spence, who dissolves in moderately strong sulphuric acid, separates from the siliceous matter by decantation or filtration through sand, adds ammonium or potassium sulphate in the necessary proportions, and then crystallises out the alum thus produced, leaving a mother liquor of phosphoric acid mixed with sulphuric acid. It is, however, very difficult to separate the whole of the alum by crystallisation from this strongly acid solution; many devices for obtaining thorough crystallisation were only partially successful, and in consequence the mother liquor of phosphoric acid is largely contaminated with sulphuric acid and considerable quantities of alumina. The alum obtained contains too much iron for the market, which renders its disposal a difficult matter.

Other methods are known for the treatment of "Redonda" by concentrated sulphuric acid, leaving the alumina and iron in an insoluble condition and separating phosphoric acid more or less mixed with sulphuric acid, but on investigation these did not give sufficient encouragement to warrant adoption, as by these processes the alumina does not seem obtainable in a marketable condition.

The "solution" processes in acid being thus eliminated, I then turned my attention to alkali.

Solution in caustic soda promised well, as sodic aluminate and sodic phosphate are formed, the iron oxide and sand being left as an insoluble residue. This method had to be abandoned, as it is covered by former patents; further, the costliness of the solvent was a serious obstacle to its commercial success.

I then turned my attention to "fusion" or furnacing methods.

The published patents of these "furnacing" methods are very numerous. They include furnacing with sodium hydrate, sodium carbonate, and sodium sulphate with and without silica, carbon, and other additions. Furnacing with sodium sulphate (salt cake) and coal was well known to me, having taken out letters patent for such a process some years before, when dealing with a kindred phosphate of alumina and iron known as "Alta Vela" phosphate.

Before going into the details of furnacing processes it is now time to consider the preparation of the raw mineral. Intimate mixture with the alkaline salt is, of course, of great importance, which necessarily pre-supposes fineness of division. "Redonda" is of a very porous texture and generally contains from 20% to 25% of moisture and combined water. In this raw state it is difficult to grind, as it binds and chokes the mills. This objection is obviated by first calcining or roasting it. There are of course many ways of performing this, but as only a low red heat is required the most economical method is the following. The rough rock mixed with about 10% of fuel, preferably coke, is thrown into a vertical cylindrical cupola, this calcines freely and thoroughly, splitting up into small lumps, which are friable, and can be ground to any desired degree of fineness through French stones; for furnacing purposes, however, it is quite sufficient to pass the calcined "Redonda" through ordinary "edge runners," together with the alkaline salt and coal or coke if required. This gives a sufficiently finely-ground and intimate mixture for practical purposes.

The intimately mixed materials, "Redonda," saltcake and coke are then thrown into a reverberatory furnace and roasted until the mass begins to "set" and get "sticky." This operation is analogous to the "black ash" operation of the Leblanc process, and takes about the same time, two hours being sufficient for each charge, about three-quarters of an hour on the "back" and one and a quarter on the "front" bed of the furnace. It requires, however, more attention from the workman, for as the mixture does not fuse together or "ball" it must be turned over at frequent intervals, so as to continually expose fresh surfaces to the action of the flames. When drawn it is a little "sticky," but on cooling breaks up into small lumps; indeed, it might be more accurately described as coarse powder. Towards the end of the furnacing operation the fire must be urged fiercely and air admitted freely that the sulphur may be oxidised to sulphurous acid and driven off, otherwise the sulphides become a serious nuisance in lixiviation. The finished charge should also be allowed to cool before being transferred to the vats. The sulphurous acid is utilised as shown below.

This method of furnacing with saltcake and coke produces, when tho' furnaced ash is lixiviated, a solution of tri-sodium phosphate and sodium aluminate, according to the following equation:—

$$AlPO_4+2(FePO_4)+SiO_2+6(Na_2SO_4)+6C$$
$$=Na_3AlO_3+3(Na_3PO_4)+6SO_2+6CO+SiO_2+Fe_2O_3,$$

leaving in the lixiviating vats an insoluble residue of sand and ferric oxide.

In this way the phosphoric acid is separated as tri-sodium phosphate and the alumina as tri-sodium aluminate, but now arises the question, "What is the value of these products?" As they stand they are unmarketable except in comparatively small quantities. It is obvious then that they must be converted into saleable articles, say alumina hydrate and calcium phosphate.

First I will deal with the alumina.

Originally I purposed treating the mixed solution of tri-sodium aluminate and tri-sodium phosphate directly with the sulphurous acid gas evolved from the furnace, precipitating alumina as hydrate and reducing the tri-sodium phosphate to di-sodium-hydrogen phosphate, with formation of sodium sulphate, according to the equation:—

$$2(Na_3AlO_3)+2(Na_3PO_4)+4(SO_2)+H_2O$$
$$=Al_2O_3+2(Na_2HPO_4)+4(Na_2SO_3),$$

and filtering off the alumina hydrate. This, however, did not answer, for I found that instead of obtaining alumina hydrate, a retrograde action took place, the phosphoric acid re-combining with the alumina, and a mixture of alumina phosphate with alumina hydrate was precipitated. The proportion of phosphoric contained in this mixture of alumina phosphate and hydrate varied considerably, but was always above 24%. Whether the lye from the vats was taken highly concentrated at say 60° Tw., or diluted down to even 10° Tw., still the alumina precipitated invariably contained phosphoric acid. This difficulty was finally overcome by the expedient of crystallising out the tri-sodium phosphate from the "vat liquor," and having thus separated the tri-sodium aluminate, treating it with the sulphurous acid gas from the furnace and precipitating the alumina hydrate. The quantity of phosphoric acid precipitating along with the alumina hydrate was, of course, dependent on the completeness of the crystallisation of the tri-sodium phosphate. By a second crystallisation, or in other words, concentrating the mother-liquor from the first crystallisation, and again crystallising out the tri-sodium phosphate, a mother-liquor of tri-sodium aluminate was obtained containing only 1·5% to 1·2% of the phosphoric acid *originally in the vat liquors*, or about 98% of the tri-sodium phosphate could be crystallised out of the vat liquor.

The tri-sodium aluminate thus obtained was then treated with the sulphurous acid gas from the furnaces by means of ordinary condensing towers, which were carefully packed with flints. The sodic aluminate liquor was forced to the top of the towers and allowed to percolate through the flints, where it met the ascending stream of sulphurous acid gas, which reacted on the liquor, precipitating alumina and leaving sodium sulphite in solution. The towers were very carefully packed, so that the precipitated alumina should not choke up the interstices between the flints, but practically there was little difficulty in keeping them clear. The alumina was then filtered at 90 to 100lb. pressure and washed in filter-presses, and dried by steam heat under a partial vacuum at a temperature not exceeding 240° F. or 116° C. It was found to be practically impossible to wash the alumina completely free from soda salts, either in the washing presses, or by agitating the alumina with successive doses of water and re-filtering.

The sodium sulphite was evaporated to dryness and utilised for fresh furnacings.

TRI-SODIUM PHOSPHATE.

The sale for this product is very limited. It was therefore decided to convert this into calcium phosphate. But this conversion could not possibly be a commercial success unless the soda were recovered in at least as valuable a form as that in which it was used for furnacing. Therefore the obvious method of treating the tri-sodium phosphate with calcium chloride, with production of sodium chloride, was out of the question.

The method of using freshly-burned lime after the manner of "causticising" was experimented with for a considerable time on the laboratory scale; taking all due precautions for obtaining well-burned lime free from carbonate, it is possible to produce a high strength calcium phosphate, containing phosphoric acid equal to from 90 to 95% tri-calcium phosphate and a thoroughly causticised solution of soda. On the manufacturing scale this was, however, found to be impossible. The best results were obtained by the device of "repeated treatments;" that is, each batch of phosphate of soda liquor was treated successively with three doses of lime, and each charge of lime was treated in turn with three successive doses of phosphate liquor, in such a way that the partially causticised liquor was finally treated with fresh lime, and the partially phosphated lime was finally treated with strong sodium phosphate, so that in the case of the liquor there was a very large excess of caustic lime, CaO, and in the case of the lime there was a very large excess of sodium phosphate present in the final treatments. In this way the liquors were fairly well causticised, but the precipitate could not be completely phosphated, the results averaging about 34% phosphoric acid, equal to 74% tri-calcic phosphate. This precipitate always contained a large proportion of *calcium carbonate*, even when the process was conducted in closed vessels and freshly and thoroughly burned lime was carefully chosen. Working in closed vessels under pressures varying up to 40lb. per square inch did not improve matters.

The use of chalk, with or without pressure, gave no better results, the average being much the same as with burnt lime. As this 74% phosphate was little better than natural phosphates, it was not considered satisfactory.

The next trial was made with calcium sulphate as gypsum. This gave somewhat better results than either lime or calcium carbonate, giving a product of from 80% to 85% tri-calcium phosphate when dried, the liquor resulting from the decomposition being, of course, sodium sulphate.

Using solid matter in suspension in water, such as lime, chalk, or gypsum, it appeared as though it were impossible to get a sufficiently intimate admixture of the sodic phosphate liquor and the suspended lime or lime-salt, so as to get thorough re-action and complete decomposition. It then became a question as to what soluble calcium salt would best produce a high-strength calcium phosphate, and at the same time produce a soda salt either of the same commercial value as salt-cake, or one that would serve equally well for future use in the furnacing operation. As before mentioned, calcium chloride was out of the question.

My friend, Mr. W. A. Hills, then suggested the use of the soluble calcium di-sulphides, obtained from old oxidised "vat waste" or "alkali waste." I have made several experiments with the "yellow liquor" produced by treating old "alkali waste" with water, but the calcium phosphate formed was always contaminated with more or less sulphur. This led me to consider the idea of "blowing" or further oxidising the "alkali waste" until the di-sulphides were converted in hypo-sulphites or thio-sulphates. In the oxidising process for the recovery of sulphur, the waste is blown until the calcium sulphides is converted into di-sulphide and thio-sulphates, but for this purpose I continued the "blowing" or oxidation until the di-sulphides were converted to the fullest practicable extent into thio-sulphates, and on lixiviation yielded, not the "yellow liquors" of the sulphur recovery process, but a colourless solution of calcium thio-sulphate. If on lixiviation of the oxidised alkali waste it was found that the liquors still contained any appreciable quantity of calcium di-sulphide, then sulphurous acid gas was forced into the liquors until the di-sulphide was converted, thus—

$$CaS_2 + SO_2 + H_2O = CaS_2O_3 + H_2S.$$

This solution of sodium thio-sulphate re-acts perfectly on a solution of tri-sodium phosphate, producing a tri-calcium phosphate of about 96% and sodic thio-sulphate, which on evaporation can be utilised for furnacing.

Here I may call attention to a somewhat remarkable re-action noticed during the experiments, which will, I believe, be novel to many. As before remarked, it was originally intended to treat the vat liquors directly with sulphurous acid gas from the furnace and thus precipitate alumina, at the same time reducing the tri-sodium phosphate to di-sodium-hydrogen phosphate and forming sodium sulphite, but this method was afterwards abandoned when it was found that alumina phosphate was precipitated with the alumina hydrate. While working on this principle, however, another difficulty arose, caused by this peculiar re-action. The mixture of sodium sulphite and di-sodium-hydrogen-phosphate was treated in the ordinary way with a sufficient quantity of calcium thio-sulphate to form di-calcium phosphate, on the supposition that the calcium thio-sulphate would decompose the di-sodium-hydrogen-phosphate, but would have *no* action on the sodium sulphite, thus precipitating di-calcium phosphate and leaving a mixture of sodium sulphite and thio-sulphate. This re-action did indeed occur, but it was accompanied by a re-action between the sodium sulphite and the calcium thio-sulphate, precipitating calcium sulphite and forming sodic thio-sulphate, so that a mixture of calcium sulphite and di-calcium phosphate was precipitated, leaving sodium thio-sulphate in solution, thus—

$$Na_2SO_3 + Na_2HPO_4 + 2(CaS_2O_3) = CaSO_3 + CaHPO_4 \\ + 2(Na_2S_2O_3).$$

As a mixture of calcium sulphite and di-calcium phosphate is of no great market value, some method had to be adopted to prevent this production of calcic sulphite. This was effected by injecting air through the mixed sodium sulphite and di-sodium hydrogen phosphate until the sodium sulphite was "blown" to sodium sulphate, which is not decomposed by calcium thio-sulphate, the re-action then taking place being—

$$Na_2SO_4 + Na_2HPO_4 + CaS_2O_3 = CaHPO_4 + Na_2S_2O_3 \\ + Na_2SO_4.$$

The di-calcium phosphate was then filtered and washed, and the mixed sodium sulphate and thio-sulphate evaporated to dryness for future use.

When it was found that the tri-sodium phosphate must be separated from the sodium aluminate by crystallisation this difficulty was partially obviated, as to produce tri-calcium phosphate it was only requisite to re-dissolve the crystals and treat the solution with the proper proportion of calcium thio-sulphate to supply three equivalents of calcium for each equivalent of phosphoric acid present. If di-calcium phosphate, however, was required, then the solution of the tri-sodium phosphate crystals was reduced to di-sodium-hydrogen-phosphate by the sulphurous acid gas, the sodic sulphite oxidised by injected air to sodic sulphate, and the mixture of di-sodium-hydrogen-phosphate and sodium sulphate treated with sufficient calcium thio-sulphate to supply two equivalents of calcium for each equivalent of phosphoric acid. The di-calcium phosphate was then filtered at 90lb. pressure and well washed, and there was no difficulty in washing this precipitate practically free from soda salts. The phosphate was then dried, like the alumina hydrate, by a steam heat under a partial vacuum, temperature not exceeding 240° F. or 116° C. By these processes the alumina is obtained as hydrate, and the phosphoric acid as di-calcium phosphate, with recovery of soda as sodium sulphate sulphite and thio-sulphate, which, when evaporated to dryness, are available for use in future furnacings.

This process, however, had certain objections. First, there was the carriage of alkali waste; then in shooting the waste into the vats (furnished with perforated false bottoms for "blowing" or oxidising) it was found that it clogged and caked in masses, so that the air could not penetrate equally through the bulk, and consequently large quantities entirely escaped oxidation; so that, as a practical result, the quantity of lime obtained as calcium thio-sulphate was about 15% to 20% on the total alkali waste. This small yield was only attained by a heavy outlay in labour, and rendered the process practically inoperative unless it could be carried on in close contiguity to an alkali works, where the waste could be "blown" without removal from the vats. Not only does this save the labour of moving the waste, but, owing to the porous or honeycombed condition in which the waste is left, it permits free access of air all through the mass, so that the sulphides are more completely oxidised, and the proportion of soluble calcium thio-sulphate obtained is larger.

While this method was being elaborated I carried out numerous experiments for obtaining a re-action between the "Redonda" phosphate and sodium chloride. Direct fusion of the phosphate of alumina and iron with sodium chloride gave no result beyond a small liberation of hydro-chloric acid gas. It then occurred to me that if free hydrogen were present to combine with the chlorine of the salt a re-action might be established, which proved to be the case.

The cheapest and most practical way of presenting free hydrogen to a mixture of phosphate and salt seemed to be to decompose steam by means of red-hot coke, while the combustion of the coke would serve to raise the temperature to the necessary degree, the coke thus serving a double purpose. Coke was used in preference to coal, that the escaping hydrochloric acid gas might be kept clear for afterwards precipitating the alumina.

A mixture in the proper proportions of phosphate, salt, and coke was now prepared, ground and mixed together in edge runners, moistened, laid out in thin cakes, and dried. The cakes were then stacked in a small cylindrical furnace of 3 feet internal diameter and 6 feet high, lined with 9in. brickwork, and enough coke added to serve as fuel, and fired. When the mass got red-hot a current of steam and air was forced through. After repeated trials good results were obtained, and it was found that on an average, 90% of the phosphoric acid in the "Redonda" was converted into tri-sodium phosphate. The re-actions are expressed by the equation—

$$\mathrm{AlPO_4 + 2(FePO_4) + 12(NaCl) + 6(H_2O) + SiO_2 + xC + xO}$$
$$\mathrm{= Na_3AlO_3 + 3(Na_3PO_4) + 12(HCl) + Fe_2O_3 + SiO_2 + xCO.}$$

On lixiviation the tri-sodium phosphate and sodium aluminate are dissolved out from the "ash," and the tri-sodium phosphate separated from sodium aluminate by crystallising as before described. The mother-liquor, consisting of sodium aluminate, is decomposed by the hydrochloric acid gas, precipitating alumina and reproducing sodium chloride. This decomposition is effected in condensing towers, in the same way as previously described. The alumina hydrate is then filtered and washed, the sodium chloride evaporated for use in a fresh furnacing. With the use of sodium chloride the production of calcium phosphate is much simplified, for calcium chloride can now be used to decompose the sodium phosphate. To produce tri-calcium phosphate, the crystals of tri-sodium phosphate are re-dissolved and calcium chloride added so that three equivalents of calcium are present for each equivalent of phosphoric acid. To produce di-calcium phosphate the dissolved tri-sodium phosphate crystals are first treated with sufficient hydrochloric acid to take up one equivalent of soda, producing di-sodium hydrogen-phosphate and chloride of sodium, and the mixture then has calcium chloride added in the proportion of two equivalents of calcium to one of phosphoric acid, producing a semi-crystalline di-calcium phosphate, which is filtered and washed in the ordinary way. This precipitated di-calcium phosphate is of very high strength, containing about 95% of di-calcium phosphate, or about 50% of phosphoric acid, equal to 109% tri-calcium phosphate.

It may be remarked here that calcined "Redonda" phosphate is freely soluble in ammonium citrate, and when applied, thus calcined and finely ground, as a fertiliser to the soil has been found of great efficiency, especially with roots and grass.

This method could be indirectly applied to the recovery of the phosphoric acid in the "basic slags" of the Thomas Gilchrist process. The slags could not be furnaced directly with sodium chloride in an atmosphere of hydrogen, or with sodium sulphate and carbon, for the sodium phosphate thus formed could not be extracted from the "ash" by lixiviation, as the caustic lime present in the ash would decompose it, precipitating calcium phosphate in the vats, along with the oxides of iron and manganese. By the methods of Messrs. Gilchrist Thomas and Twynam, however, the slag is dissolved in hydrochloric acid and a large proportion of the phosphoric acid precipitated from the solution as ferric phosphate; and this ferric phosphate is then decomposed by strong sulphuric acid, forming a mixture of free phosphoric and sulphuric acids and sulphate of iron, which is insoluble in excess of strong sulphuric acid, and can be separated by filtration. It appears to me that it would be more economical to recover the phosphoric acid of this ferric phosphate, in the form of *tri-sodium phosphate*, by furnacing with salt in the presence of hydrogen, and decomposing the tri-sodium phosphate by calcium chloride, precipitating calcium phosphate and recovering sodium chloride for use in future furnacings.

Manchester Section.

Chairman: Sir H. E. Roscoe.
Vice-Chairman: D. B. Hewitt.
Committee:

R. F. Carpenter.
C. Estcourt.
H. Grimshaw.
Peter Hart.
J. von Hohenhausen.
T. Jackson.

J. Knowles.
I. Levinstein.
C. Schorlemmer.
Watson Smith.
Wm. Thomson.
D. Watson.

Local Secretary:
J. Carter-Bell, Bankfield, The Cliff, Higher Broughton, Manchester.

Notices of papers and communications for the meetings to be sent to the Local Secretary.

Meeting held Tuesday, February 24, 1885,

SIR HENRY ROSCOE IN THE CHAIR.

The Chairman: Before beginning the usual business of the meeting, I wish to state that an international committee has been formed for the purpose of erecting a statue to the memory of Nicolas Leblanc, the well-known discoverer of the Leblanc process of alkali making. A large number of Continental scientific men and chemical manufacturers has been appointed a committee for the purpose of endeavouring to obtain subscriptions, with the addition of Colonel Gamble, Mr. Weldon and myself, as representing this country. I feel sure that every member of this Society is so well acquainted with the claims of Nicolas Leblanc, and with the enormous value of his discovery to English industries, that I believe you will all agree with me in thinking that it would be only proper for this Section in particular, situated in the midst of a district which has profited so largely by this discovery, to send our acknowledgments to the committee, and assist them in helping forward the movement. I shall be glad to receive any subscriptions to the fund, and beg to commence the list with a contribution of five guineas.

THE HISTORY OF CREOSOTE, CEDRIRET AND PITTACAL.

BY PROFESSOR C. SCHORLEMMER, F.R.S.

CREOSOTE was discovered by Reichenbach[*] in the year 1832; he found it in the tar, as well as in the pyroligneous acid obtained by the dry distillation of beechwood, and describes it as a colourless liquid, which strongly refracts light, and begins to boil at 203° It has an unpleasant, penetrating smell, reminding one at the same time of that of smoked meat, and possesses the characteristic property of coagulating albumin. Meat, after being dipped in an aqueous solution of creosote for half an hour or one hour, may be dried in the open warm air of a summer's day without undergoing putrefaction. He showed samples of meat thus prepared at the meeting of the German Naturalists in Vienna, and people who had made many voyages found it very tasty.

Reichenbach continues: "Now, as pyroligneous acid by itself produces this effect, as well as the tar-water—which, according to public statements, may be prepared by lixiviating the soot of our stoves and chimneys—there cannot be any doubt that creosote is the *antiseptic, conservative principle* contained in these liquids as well as in smoke. As we further know from Pliny's tales that the Egyptians prepared their mummies with pyroligneous acid, which they obtained by carbonising the aromatic woods of their hot climate, we perceive that the ultimate cause dwells in the new body, creosote being the mummifying element. The so-called costly spices used by the ancients at the same time, were probably nothing but an expedient to mask with other perfumes the unpleasant empyreumatic smell dwelling in all the products of carbonisation."

Of the word *creosote*, which he proposed as the name for the new substance, he says: "I derive it from its property to preserve meat, being the most striking, the most peculiar, and that which has been known from the most ancient times. κρέας means in Greek flesh, the genitive being κρέατος, or κρέαως, and contracted κρέως; σώζω means, I preserve or save; both words may etymologically be combined to the word *creosote*, meaning the meat preserving or saving from decay.

Very soon after, Runge[†] discovered carbolic acid in coal-tar, and Laurence[‡] obtained from it his hydrate of phenyl, which later on was found to be identical with Runge's compound. But as to the relation existing between creosote and carbolic acid no clear views were held, and a confusion arose, which was kept up with an obstinacy unparalleled in the history of our science, and lasting for many years.

Runge described carbolic acid as an oily, very refractive liquid, which, like creosote, produces a white spot on the skin, after which it becomes glistening and peels off. Like creosote, it precipitates albumin and protects organic bodies from putrefaction, but it does not appear to be identical with smoky principle, inasmuch as meat preserved by it has an abominable taste. It differs, further, from creosote by being an acid and forming salts.

In spite of this, Reichenbach thought to recognise his creosote in carbolic acid; he was the more inclined to do so as he had previously stated that creosote was also found in coal-tar, animal-tar and amber-tar, almost as abundantly as in wood-tar.[§]

Runge could not agree to that; he maintained the peculiarity of carbolic acid, and described the properties by which the two bodies differed as exactly and correctly as it was possible at that time.[‖]

Laurent mentions in his well-known Memoir the great resemblance of phenylhydrate with carbolic acid and creosote, and says: "It is even probable that Runge's carbolic acid is nothing but impure phenylhydrate." Creosote, however, was regarded by him as a totally different body, on account of its *higher boiling point, its inability to crystallise*, even at 27°, while phenylhydrate forms crystals melting at 34° to 35°, and carbolic acid when cooled down deposits sometimes needles melting above 15°. Chlorine converts creosote into a brown substance, containing no chlorophenisic acid (trichlorophenol), which is readily obtained from phenylhydrate. Moreover, the action of bromine, nitric acid and ferric chloride on the latter compound is quite different from that on creosote.

These distinct statements did not prevent Gmelin to refer in his hand-book to Laurent's *investigation*, in order to prove his preconceived opinion that creosote, carbolic acid and phenylhydrate were identical, and differing only by a certain degree of purity.

Gmelin's views were accepted by most chemists, the more readily as Reichenbach himself believed in the identity of creosote and carbolic acid, and stated that he had obtained the former from coal-tar.

Soon carbolic acid was found in commerce under the name of creosote; the few chemists who were in possession of the genuine article could easily convince themselves of the peculiarity of creosote, but this was not sufficient to produce a change in the general accepted view. If about 40 years ago a chemist ordered creosote from a German firm he always obtained carbolic acid.[¶]

About that time, Gorup-Besanez examined a sample of "crystallised creosote," and compared it with one from Blansko in Moravia, where Reichenbach had discovered it. He found both bodies totally different, but he had not sufficient material for a more exhaustive investigation.[**]

A few years after he obtained again genuine creosote from Blankso through Batka in Prague, *and found that it did not give picric acid by the* action of nitric acid, and chlorine did not convert it into chlorophenisic acid, which can so easily be obtained from carbolic acid. On treating creosote with hydrochloric acid and potassium chlorate, no chloranil $C_6Cl_4O_2$ was formed, but a similar body containing *hydrogen*.[††]

Hardly any attention was paid to these observations; Gmelin mentions them in his hand-book, only to add that if Gorup-Besanez wanted to prove the existence of creosote, he ought to have examined that which he had prepared himself, and not a sample obtained from a wholesale druggist.

Two years later, Gorup-Besanez published a more extended investigation, in which he confirmed the statements of Reichenbach, Runge and Laurent. The portion boiling at 203·5 to 208° had the same composition as the creosote sent by Reichenbach to *Liebig, and which Ettling had analysed*. By the action of chlorine he obtained bodies, which he called

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[*] Schweigger's Neues Jahrbuch Chem. Phys. 6, 301, 351; 7, 1, 57; 8, 57, 399.

[†] Pogg Ann. 31, 69.

[‡] Ann. Chim. Phys. (3), 30, 195.

[§] Pogg. Ann. 31, 497.

[‖] *Ibid.* 32, 306 and 328.

[¶] Mohr, Comment. Deutsch. Pharm. 1863, 415.

[**] Ann. Chem. Pharm. 113, 132.

[††] *Ibid.* 78, 231.

chlorinated xylones, and formulated as *pentachlorxylone*, $C_{13}H_7Cl_5O_3$, and *hexchlorxylone*, $C_{13}H_6Cl_6O_3$. He pointed out that they bore great resemblance to Städeler's chlorinated quinones.*

Although this research did not clear up the chemical nature of creosote, yet it furnished a complete proof of the fundamental difference between creosote and carbolic acid.

At the same time Völkel published a paper on the same subject; he examined creosote which was manufactured from wood-tar in Solothurn. He purified it by repeated treatment with boiling potash solution, precipitation by sulphuric acid and subsequent distillation. The oily liquid thus obtained boiled at 202° to 208°, and exhibited the general properties of Reichenbach's compound; differing from it, however, by its higher specific gravity and its composition,† as the following results of the analyses of Ettling, Gorup-Besanez and Völkel show:—

	Creosote from Blansko (mean of 8 analyses).	Creosote from Solothurn (mean of 3 analyses).
Carbon	75·21	72·45
Hydrogen	7·90	7·10
Oxygen	16·89	20·45
	100·00	100·00

Völkel's creosote was therefore decidedly different from Reichenbach's, and his results, instead of contributing to the solution of the creosote question, made it more complicated, the more so as Deville, who analysed creosote which he had obtained from Reichenbach, and such which Pelletier had prepared, had got results agreeing with those of Völkel.‡

Völkel was of opinion that Gorup-Besanez's creosote had not been sufficiently purified, wherefore the latter subjected 500 grammes to the treatment, which Völkel had employed until only a few grammes were left. The amount of carbon had now diminished 1·5, and that of hydrogen only 0·2 per cent.

The smell of the purified product resembled that of guaiacol, which is obtained by the distillation of guaiacum resin. Gorup-Besanez says that the resemblance of guaiacol with creosote had already been noticed, and it appeared to him probable that from the latter by repeated treatment with potash guaiacol might be obtained.§

In the same year (1855) Williamson published the results of experiments which Fairlie had made, in order to find whether coal-tar creosote consisted of carbolic acid.|| He thus discovered cresyl hydrate C_7H_8O, the next higher homologue of phenyl hydrate. This new compound, boiling at 203°, or almost at the same temperature as beechwood creosote, although not being mistaken for the latter, yet made the subject still more confused.

A new chapter in the history of creosote begins with Hlasiwetz's researches, "On Beechwood-tar Creosote and the Products of the Distillation of Guaiacum Resin."¶

By the action of potash on creosote from Blansko he obtained a crystalline potassium compound $C_8H_9KO_2+2H_2O$, which when decomposed by sulphuric acid yields a colourless, oily liquid ($C_8H_{10}O_2$), having an aromatic smell, a burning taste, and boiling at 219°.

The same body, which he called *creosote-guaiacol* or *creosol*, was found in the products obtained by distilling guaiacum resin, together with its lower homologue *guaiacol* $C_7H_8O_2$, which boils at 200°, and which had already been obtained by Deville and Pelletier.

Hlasiwetz was led to the conclusion that wood-tar creosote is decidedly not impure phenyl-hydrate, as Gmelin assumed; it is also totally different from coal-tar creosote, consisting chiefly of cresyl alcohol. Most probably it is a kind of ether corresponding to the potassium compound, and containing the radical C_9H_{11}, which would simply explain why creosote contains a larger percentage of carbon than creosol.

In the following year Duclos published a paper on cresyl alcohol, which he had obtained from coal-tar, but also found in the tar of the Giessen Gasworks, where only wood, chiefly pinewood, was used.**

This is, as Gorup-Besanez pointed out, in contradiction to all we know of the products formed by the distillation of wood. Neither he nor other chemists had succeeded in detecting phenols in wood-tar. If Duclos found such bodies we must come to the conclusion that pinewood yields products totally different from those obtained from beechwood.

Meanwhile Gerhardt†† had recalculated the formulæ of the chlorinated xylones in $C_8H_5Cl_3O_2$ and $C_8H_4Cl_4O_2$, regarding them as the homologues of the chlorinated quinones. Hlasiwetz's results gave a support to the new formulæ, and Gorup-Besanez intended to commence a new investigation, but was prevented, inasmuch as he could not get a new supply of creosote from Blansko; this source was exhausted, and different samples which he obtained from other sources under the name of beechwood creosote were found to be nothing but carbolic acid.

In 1864, Hugo Müller brought an important contribution to the history of our subject. He examined English creosote, which was prepared in London from *Stockholm-tar*, and appeared, as he wrote to Gorup-Besanez, to be identical with that from Blansko. He isolated from it Hlasiwetz's creosol, which by the action of iodine, phosphorus, and water, or hydriodic acid, was resolved into *methyl iodide* and *homopyrocatechin* $C_7H_8O_2$, the higher homologue of pyrocatechin $C_6H_6O_2$,‡‡

$$C_8H_{10}O_2+HI=C_7H_8O_2+CH_3I.$$

The investigations of Völkel, Gorup-Besanez, Hlasiwetz, and Müller, who had examined wood-tar creosote from three different sources, furnished the most convincing proofs that it has nothing in common with coal-tar creosote. In the way of a general agreement, there remained only the statement of Reichenbach that coal-tar contained creosote, and that of Duclos that wood-tar contained phenols.

Reichenbach, however, never proved his statement and as Duclos was the only chemist who found phenols in wood-tar, the great creosote question appeared now to be settled, and yet it did not come to rest.

In 1865, A. E. Hofmann examined different commercial products which he had obtained from several firms as genuine beechwood creosote. He obtained from them phenyl hydrate, and from this he prepared chloranil ($C_6Cl_4O_2$) and picric acid. Not being acquainted with the researches of Hlasiwetz and Müller, he denied the existence of an individual creosote, it being nothing else but impure phenyl-hydrate, while the chlorinated xylones consisted of a mixture of chloranil (tetrachlorquinone) and chlorinated phenols. The names creosote and hexchlorxylone had to be removed from the list of chemical compounds, while it might be convenient to retain the name creosote for the commercial article.§§

Gorup-Besanez replied that Hofmann, in order to prove his statements, ought first to have convinced himself that he had really to do with genuine creo-

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* Ann. Chem. Pharm. 86, 223.

† *Ibid.* 86, 66 and 93.

‡ Gerhardt, Traité Chim. Org. 3, 20.

§ Ann. Chem. Pharm. 96, 39.

|| Journ. Chem. Soc. 7, 232.

¶ Ann. Chem. Pharm. 100, 339.

** Ann. Chem. Pharm. 109, 135.

†† Traité Chim. Org. 3, 23.

‡‡ Chem. News, 10, 260.

§§ Journ. Prakt. Chem. 98, 225.

sote. Without difficulty he might have found that the druggists whom he mentions could not, though with best intentions, have supplied him with beechwood creosote, as it had disappeared from the German market long ago. Thus he would have not come into the position to prove, what was already known for a long time—viz., that carbolic acid was sold under the name of creosote.[*]

This episode had, however, an issue as unexpected as pleasing, for Fresenius informed Gorup-Besanez that the "Verein für Chemische Industrie in Mainz" manufactured creosote from beechwood-tar, and brought it into commerce. In examining it, Gorup-Besanez found that it was different from Blansko creosote, but probably identical with that examined by Völkel, inasmuch as the chief portion boiled at 199-207°. He succeeded in isolating from it a large quantity of *guaiacol* and some *creosol*. By acting on the crude creosote with hydrochloric acid and potassium chlorate, he obtained *tetrachloroguaiacone* ($C_7H_2Cl_4O_2$) and *tetrachlorocreosone* ($C_8H_4Cl_4O_2$), the latter being identical with hexchloroxylone. These compounds behaved like homologues of tetrachloroquinone, and might therefore be regarded as derivatives of the two yet unknown homologues of quinone, *guaiacone* and *creosone*, standing in the same relation to guaiacol and creosol as quinone to hydroquinone:

Quinone	$C_6H_4O_2$	Hydroquinone	$C_6H_6O_2$
Guaiacone	$C_7H_6O_2$	Guaiacol	$C_7H_8O_2$
Creosone........	$C_8H_8O_2$	Creosol	$C_8H_{10}O_2$

On treating the crude Rhenish creosote with iodine, phosphorus and water, Gorup-Besanez obtained methyl-iodide and pyrocatechin, which undoubtedly were derived from guaiacol:

$$C_7H_8O_2+HI=C_6H_6O_2+CH_3I.$$

The latter bearing such a great resemblance to pyrocatechin, it appeared that it and creosol were the true homologues of pyrocatechin and not of hydroquinone. There was, however, one fact in opposition to this view—viz., pyrocatechin boiling at 240°, or higher than its homologues.

Gorup-Besanez found, further, that the yield of pyrocatechin and methyl iodide falls short of that indicated by the above equation, a large quantity of a thick oil, containing phosphorus, being found, possessing such an abominable smell, that he hesitated to examine it more closely, its odour adhering to the clothes most obstinately, that it made one socially impossible.

As already stated, Hlasiwetz assumed the existence of the compound $C_8H_9(C_3H_{11})O_2$ in Moravian creosote; Gorup-Besanez found that the composition of the Rhenish creosote agreed best with the assumption it contained the radical C_3H_5.[†]

This view was opposed by Marasse,[‡] because if the portion of the creosote boiling between 199-208° contained this radical, it ought by the action of potash to yield allyl alcohol or a similar compound. But such a body had not been observed. Moreover, guaiacol being undoubtedly the methyl-ether of a kind of phenol, and it being known that such bodies are not decomposed by potash, Gorup-Besanez's view can be the less maintained as he purified creosote by repeated treatment with potash and hydrochloric acid, without altering essentially its composition.

But why does the percentage composition of Rhenish creosote differ so considerably from that of guaiacol or creosol?

	Creosote.	Guaiacol.	Creosol.
Carbon	72·14	67·71	69·58
Hydrogen	7·16	6·45	7·21
Oxygen	20·70	25·81	23·18
	100·00	100·00	100·00

The most simple answer is furnished by the assumption that creosote contains, besides guaiacol and creosol, some compounds containing more carbon and less oxygen, but boiling at the same temperature as these two bodies.

Marasse was able to prove this hypothesis; he found that the portion of creosote boiling below 199° contained common phenol or phenyl hydrate C_6H_6O, and the higher boiling portion its homologues cresol C_7H_8O, or cresyl hydrate and phlorol- or phloryl-hydrate $C_8H_{10}O$, together with guacol and creosote, which are the creonomethyl ethers of pyrocatechin and homopyrocatechin, which is proved not only by their decomposition with hydriodic acid, but also by the fact that they can be obtained from the latter simply by heating them with potash and methyl iodide.

As the initial members of both series have different boiling points, that of the first member of the guaiacol-gruppe agreeing with the second of the phenol group, it becomes self-evident that the most volatile portion of creosote must consist only of common phenol:

PHENOL SERIES.	Boil. Point.	GUAIACOL SERIES.	Boil. Point.
Phenol....$C_6H_5{\cdot}OH$	181°		
Cresol ..$C_6H_4\left\{\begin{matrix}CH_3\\OH\end{matrix}\right\}$..	203	Guaiacol..$C_6H_4\left\{\begin{matrix}OH\\OCH_3\end{matrix}\right\}$..	200°
Phlorol ..$C_6H_3\left\{\begin{matrix}(CH_3)_2\\OH\end{matrix}\right\}$	220	Creosol $C_6H_3(CH_3)\left\{\begin{matrix}OH\\OCH_3\end{matrix}\right\}$	219

The creosote analysed by Gorup-Besanez was a mixture of guaiacol and creosol; one consisting of equal parts having a composition nearly agreeing with that found by him:

Carbon........................	72·41
Hydrogen	6·90
Oxygen	20·69
	100·00

Moravian creosote contains a larger percentage of carbon, being essentially a mixture of creosol and phlorol.

Marasse further proved that tetrachloroguaiacone and tetrachlorocreosone are not derivatives of guaiacol and creosote, but stand to cresol and phlorol in the same relation as chloranil or tetrachloroquinone to phenol. We call them now tetrachlorotoluquinone and tetrachloroxyloquinone or tetrachlorophlorone.

The Rhenish creosote was also examined by Bräuninger; he found in his sample only traces of phenol, less than two per cent. of cresol, and no phlorol whatever. In the sample which Gorup-Besanez had examined he also found but little cresol, and still less phlorol[§], whilst Mendelsohn and Tiemann[‖] observed a large quantity of the latter compound in the portion of Rhenish creosote boiling at 220°. Another sample which Gorup-Besanez obtained, was examined by Biechle, who found it to consist almost entirely of creosol.[¶]

Creosote is, therefore, a mixture of guaiacol and creosol in varying proportions, with larger or smaller quantities of phenols. The presence of the latter shows that beechwood creosote is more closely related to coal-tar creosote than it was formerly believed. Characteristic of the former is the presence of the guaiacols, which do not occur in coal-tar, and which are undoubtedly produced by some aromatic bodies found only in wood, and which have been examined by several chemists.[**]

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[*] Journ. Prakt. Chem. 97, 63.

[†] Ann. Chem. Pharm. 113, 129.

Ibid. 152, 59.

[§] Ann. Chem. Pharm. 185, 339.

[‖] Ber. Deutsch. Chem. Ges. 10, 59.

[¶] Ann. Chem. Pharm. 151, 101.

[**] Meissner and Sheppard, Untors. Enstehung. Hippurs. Hannover, 1867; Erdmann, Ann. Chem. Pharm. Suppl. 5, 223; Bente, Ber. Deutsch. Chem. Ges. 8, 476; Singer, Monatsh. Chem. 3, 395; Bevan and Cross, Chem. News 42, 47, and 91; 41, 61; Journ. Chem. Soc., 1883. 1, 18.

Creosote contains, besides phenols and guaiacols, some bodies which are insoluble in alkalis. Mendelsohn and Tiemann[*] found *homopyrocatechin-dimethylether*, or *methylcreosol*, $C_6H_3(CH_3)(OCH_3)_2$, which boils at 214-218°, whilst, according to Hofmann[†], the higher boiling portion contains the following compounds, which dissolve in alkalis:

Pyrogallol-dimethylether	$C_6H_3(OCH_3)_2OH$
Methylpyrogallol-dimethylether	$C_6H_2(CH_3)(OCH_3)_2OH$
Propylpyrogallol-dimethylether	$C_6H_2(C_3H_7)(OCH_3)_2OH$

These researches cleared up the chemical nature of two interesting compounds, which were also discovered by Reichenbach.

Cedriret does not exist, ready formed, in wood-tar, but is obtained from it, according to Reichenbach, by the following process: The rectified oil, which is obtained by distilling the tar, is freed from acetic acid by means of carbonate of potash, and then treated with a solution of caustic potash. The alkaline solution is saturated with acetic acid, by which some oil separates out, while another portion is obtained by distilling the aqueous solution. When about one-third has passed over, that which follows is tested with a solution of ferric sulphate. As soon as this produces a red precipitate—the distillate is collected separately. All bodies which easily part with oxygen produce this red colouration; as, for instance, a solution of chromate potash and tartaric acid; on adding it a red precipitate, consisting of needles, appears after five minutes, and soon fills the whole liquid, from which it slowly deposits, leaving a colourless solution. The same change is produced by the oxygen of the air.

This body is cedriret, which name is composed of *cedrium*, tar-water, and *rete*, net, because its crystals group themselves on the filter like a net.[‡]

According to Reichenbach, it is insoluble in water, spirits of wine, and all ethers, but dissolves in pure sulphuric acid, containing no nitric with an indigo blue—and in creosote with a purple colour.

Alcohol separates it from the latter solution in the form of a crystalline precipitate.

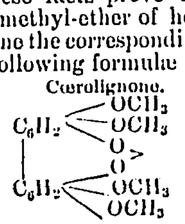
Berzelius says that it is only obtained in a small quantity; and its preparation requires that skill in the treatment of empyreumatic bodies in which its discoverer has so often proved his mastership.

Völkel did not succeed in obtaining it from the wood-tar which he examined.[§]

In the year 1872, Liebermann[‖] examined a blue substance, which he called *cœrolignone* $C_{16}H_{16}O_6$, and which obtained by purifying pyroligneous acid by means of potassium bichromate.[‖] He found it to be insoluble in most solvents; sulphuric acid dissolved it with a cornflower-blue colour and phenol with a red colour. Alcohol or ether precipitate it from the latter solution in beautiful steel-blue needles. Reducing agents convert it into colourless *hydrocœrolignone* $C_{16}H_{18}O_6$, crystallising in monoclinic prisms. On heating the latter with hydrochloric acid to 200°, it is resolved into methyl chloride and *hexhydroxydiphenyl* $C_{12}H_4(OH)_6$, which, by red-hot zinc dust, is reduced to diphenyl $C_{12}H_{10}$.

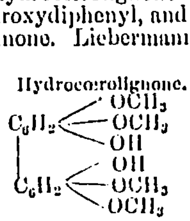
These facts prove that hydrocœrolignone is the tetramethyl-ether of hexhydroxydiphenyl, and cœrolignone the corresponding quinone. Liebermann gives the following formulæ:—

Cœrolignone.



$$\begin{array}{l} C_6H_2\left\langle\begin{array}{l}OCH_3\\OCH_3\\O\end{array}\right. \\ \;| \qquad\qquad\quad > \\ C_6H_2\left\langle\begin{array}{l}O\\OCH_3\\OCH_3\end{array}\right. \end{array}$$

Hydrocœrolignone.



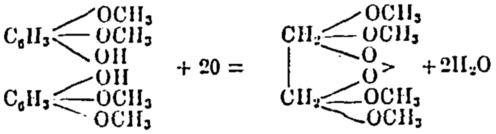
$$\begin{array}{l} C_6H_2\left\langle\begin{array}{l}OCH_3\\OCH_3\\OH\end{array}\right. \\ \;| \\ C_6H_2\left\langle\begin{array}{l}OH\\OCH_3\\OCH_3\end{array}\right. \end{array}$$

Marx,[¶] and afterwards Hofmann,[**] pointed out that Liebermann's compound was nothing else but Reichenbach's cedriret. Hofmann obtained it by the action of potassium chromate or nitric acid on a high boiling oil from wood-tar, in needles having a violet lustre, and says Reichenbach had described it, not only with a precision which leaves nothing to wish for, but also so attractively, that one must be astonished how a body possessing such charming properties *had been neglected for thirty years—nay*, almost entirely forgotten.[††]

Liebermann recognised the identity of cedriret with cœrolignone, to which he had given this name on account of its blue colour, of its being obtained from wood, and possessing the characteristic properties of a quinone. He says that as Reichenbach had described it as a red body obtained from a high boiling oil, from which neither Völkel nor he were able to prepare it, while cœrolignone had a blue or violet colour, and was derived from pyroligneous acid, he had formerly come to the conclusion that cedriret, *if* existing, would probably be nearly allied to, but not identical with, cœrolignone.[‡‡]

Hofmann, who prefers Reichenbach's shorter and very characteristic name, found afterwards that cedriret is a derivative of the dimethyl-ether of pyrogallol. On adding potassium bichromate to its solu*tion in acetic acid, the liquid becomes hot, and some* is filled with needles, which, in transmitted light, appear red, and in the reflected splendidly steel-blue. It may also conveniently be prepared by adding hydrochloric acid to a boiling alkaline solution of potassium chromate and pyrogallol-dimethylether, the liquid changing into a mass of slender reddish-brown needles.[§§]

Its formation is explained as follows:—



$$\begin{array}{l} C_6H_3\left\langle\begin{array}{l}OCH_3\\OCH_3\\OH\end{array}\right. \\ C_6H_3\left\langle\begin{array}{l}OH\\OCH_3\\OCH_3\end{array}\right. \end{array} + 20 = \begin{array}{l} CH_2\left\langle\begin{array}{l}OCH_3\\OCH_3\\O\end{array}\right. \\ \;| \qquad\qquad\quad > \\ CH_2\left\langle\begin{array}{l}O\\OCH_3\\OCH_3\end{array}\right. \end{array} + 2H_2O$$

Pittacal.—Reichenbach observed that the oil from wood-tar, which is heavier than water, after being freed from acids by caustic potash, yields, when stirred up with baryta water or baryta hydrate, a blue colour, which gradually changes into black. The new body *thus formed* he called pittacal (ἡ πεττα το καλλον), but gives no particulars as to its mode of purification, etc., reserving it for a further communication. He describes it in the following words: "When obtained from a solution as a precipitate or separated by evaporation, it unites to a hard, brittle, dark-blue mass, having a dull fracture; like indigo, it assumes a metallic reflection on rubbing, which varies from a coppery lustre to a pure brass-yellow, according to its purity, being in the mean a splendid golden yellow, in striking contrast to the dark blue ground. This golden lustre appears also on allowing the moist mass to dry up. When pure, this lustre predominates in such a way that it cannot be obtained without it; everything moistened by it—porcelain, glass, paper, linen, etc.—appears gilded on a blue ground.

"In the pure state it is taken up by water; on standing, the dilute solution deposits dark violet flakes, and becomes colourless; but if concentrated nothing separates out. It dissolves freely in acetic acid, with an auroral colour; alkalis throw it down again. On pouring an aqueous solution into concentrated potash-lye, the bright blue colour changes into a violet blue, and after some time blue flakes separate, consisting of fine needles.

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[*] Ber. Deutsch. Chem. Ges. 8, 1130; Tiemann and Koppe, *Ibid.* 11, 2005.

[†] *Ibid.* 7, 78; 8, 60; 11, 329 and 1155; 12, 1371 and 2216.

[‡] Berzelius, Jahresb. 15, 408.

[§] Ann. Chem. Pharm. 80, 102.

[‖] *Ibid.* 169, 221.

[¶] Wagner's Jahresb. 1872, 827.

[**] Ber. Deutsch. Chem. Ges. 8, 68.

[††] *Ibid.* 7, 78.

[‡‡] *Ibid.* 8, 96.

[§§] Ber. Deutsch. Chem. Ges. 11, 329.

"It combines with metallic oxides, and gives with sugar of lead, tin-salt, sulphate of copper ammonia, acetate of alumina, blue precipitates having a violet lustre."

Reichenbach adds: "It thus appears to have a technical value, which for Europe, where no indigo is produced, may become of an incalculable importance. Preliminary experiments have shown that it may be fixed on cotton or linen by means of acetate of alumina or tin-salt, producing a fast blue, not being affected by light, water, soap, ammonia, urine or wine."[*]

Reichenbach did not publish any more on this subject; nor did Völkel succeed in preparing it,[†] and it was almost forgotten, till Grätzel, a chemical manufacturer in Hannover, re-discovered it. On oxidising the high-boiling fraction of wood-tar oil he obtained a colouring matter, which he gave to Wichelhaus for further examination.[‡]

On purifying the crude product a compound was obtained crystallising from alcohol or dilute acetic acid in orange needles, dissolving in ammonia with a blue, and in the alkalis with a purple colour. On allowing the solution to remain in contact with air, or more quickly on passing carbonic acid into it, blue salts are precipitated, which also occurs on the addition of salt-solutions. They are soluble in pure water. If the precipitation proceeds slowly a skin is first formed on the surface, exhibiting a splendid metallic lustre. The salts of calcium, barium, magnesium, and tin give blue precipitates, often showing a golden reflection; those produced by calcium and magnesium being soluble in pure water.

The compound dissolves in concentrated sulphuric acid with a purple colour, changing by heat into a bright, pure blue.

The orange crystals are not Reichenbach's pittacal; the latter consisting of the salts of this body, which Wichelhaus, in order to avoid mistake, and in remembrance of Reichenbach's body, called *eupitton* (ευ, beautiful).

Hofmann [§] then found that this compound, which he calls *eupittonic acid*, $C_{25}H_{26}O_9$, is obtained by heating the dimethyl-ether of pyrogallol with hexchlorethane and alcoholic potash to 160° to 170°:—

$$3C_6H_3(OCH_3)_2OH + C_2Cl_6 + 7KOH =$$
$$C_{19}H_8(OCH_3)_6O_3 + CHKO_2 + 6KCl + 5H_2O.$$

It is also produced by melting a mixture of the dimethyl-ethers of pyrogallol and methylpyrogallol with caustic soda in the presence of air [||]:—

$$2C_6H_3(OCH_3)_2OH + C_7H_5(OCH_3)_2OH + 2NaOH + 3O =$$
$$C_{19}H_6Na_2(OCH_3)_6O_3 + 5H_2O$$

Reichenbach obtained from the oils of wood-tar a liquid, which he called *picamar (pice amarum)*. It boiled at 285°; had a faint but unpleasant smell, and a bitter, burning taste, which afterwards becomes cooling like that of peppermint. Its alcoholic solution gave with baryta-water pittacal, and from this it appears that his picamar contained the dimethyl-ethers mentioned above.

Eupittonic acid contains two atoms of hydrogen, which can be replaced by metals. On adding caustic soda to its alcoholic orange solution it changes into splendid blue colour, a flocculent precipitate of *sodium eupittonate* $C_{19}H_6O(OCH_3)(ONa)_2$ separating out, which after 24 hours is converted into a mass of small prisms having a beetle-green lustre. *Barium eupittonate* $C_{19}H_6O(OCH_3)_6O_2Ba$ is a precipitate consisting of blue needles, while the lead-salt crystallises in pale reddish needles.

At the same time when Reichenbach carried out his researches at Blansko, Runge in Berlin examined the constituents of coal-tar, in which he discovered carbolic acid, as already mentioned. On treating coal-tar oil with milk of lime, and allowing the solution to remain in contact with air, it coloured red, a compound being formed which he called rosolic acid.[¶] It is easily seen that the formation of this body is quite analogous to that of pittacal. But the constitution of these two compounds is also perfectly analogous, rosolic acid or *aurin*, as it is now called, being obtained by heating carbolic acid with oxalic and sulphuric acids:—

$$3C_6H_6O + C_2H_2O_4 = C_{19}H_{14}O_3 + CH_2O_2 + 2H_2O.$$

Now eupittonic acid is *hexmethoxylaurin*—

$$C_{19}H_8(OCH_3)_6O_3,$$

which is proved by its formation as well as by its properties. On heating aurin with ammonia, it is converted into pararosaniline:— [**]

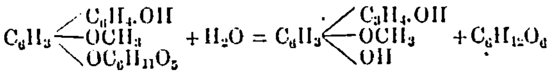
$$C_{19}H_{14}O_3 + 3NH_3 = C_{19}H_{17}N_3 + 2HO.$$

In the same way Hofmann converted eupittonic acid into *hexmethoxylpararosaniline*—

$$C_{19}H_{11}(OCH_3)_6N_3,$$

which dyes silk, sulphurated wool, and tannated cotton a fine and pure blue.

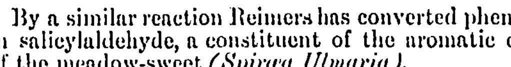
I have already stated that the guaiacols, as well as the other methyl-ethers contained in wood-tar, owe their origin to certain aromatic compounds contained in wood. Although we know but little of them, we have got a clue to their formation. The cambial liquid of the coniferæ, from which in spring and early summer the new wood is formed, contains the glucoside *coniferin* $C_{16}H_{22}O_8$, which, as Haarmann and Tiemann[††] have shown, is by the action emulsin, a ferment contained in almonds, resolved into grape sugar and coniferyl alcohol:—



$$C_6H_3\begin{cases}C_3H_4.OH\\ OCH_3\\ OC_6H_{11}O_5\end{cases} + H_2O = C_6H_3\begin{cases}C_3H_4.OH\\ OCH_3\\ OH\end{cases} + C_6H_{12}O_6$$

Coniferyl alcohol, as well as coniferin, are converted by oxidation into *vanillin*[††], which gives to vanilla its delightful smell.

The same compound may, as Reimers and Tiemann found, be obtained from wood-tar, it being formed by heating guaiacol with chloroform and caustic potash[‡‡]:—



$$C_6H_4\begin{cases}OCH_3\\ OH\end{cases} + CHCl_3 + 3KOH = C_6H_3\begin{cases}OCH\\ OCH_3\\ OH\end{cases} + 3KCl + 2H_2O.$$

By a similar reaction Reimers has converted phenol in salicylaldehyde, a constituent of the aromatic oil of the meadow-sweet *(Spiræa Ulmaria)*.

I may further mention that Griffiths has found phenol in the stem leaves and cones of the Scotch fir *(Pinus sylvestris)*.[§§]

Pelletier and Walter, in the introduction to their Memoir on the chemical investigation of the products formed in the preparation of illuminating gas from resin, say that chemical operations which are carried on a large scale permit the observation of certain phenomena, and the investigation of certain laws, as well as the discovery of new products, which by working in the laboratory would have escaped our notice. Chemical industry, which, in its advance, uses theories as scouts, thus pays back its debt to the chemists who have given assistance in furnishing them with new compounds for their examination, and thus extending the sphere of our science.

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[*] Schweigg. Neu. Jahrb. Chem. Phys. 8, 1.

[†] Ann. Chem. Pharm. 80, 103.

[‡] Ber. Deutsch. Chem. Ges., 9, 331; Grätzel, *Ibid.* 11, 2085.

[§] *Ibid.* 11, 1115.

[||] Ber. Deutsch. Chem. Ges. 12, 1371 and 2216.

[¶] Pogg. Ann. 31, 70.

[**] Dale and Schorlemmer, Journ. Chem. Soc., 1877, 2, 121.

[††] Ber. Deutsch. Chem. Ges. 8, 609.

[‡‡] *Ibid.* 9, 123.

[§§] Chem. News, 19, 95.

Thus the manufacture of coal-gas has made chemists acquainted with some new bodies of the greatest interest. Without gas-lighting we should probably have yet to discover Faraday's liquid hydrocarbons (butylene and benzene), Kidd's naphthalene, and Dumas and Laurent's para-naphthalene (anthracene).

It is scarcely necessary for me to enumerate here how many new and important bodies have since been obtained from coal-tar. This black, fœtid substance, formerly a large, embarrassing bye-product, has now become of the highest importance for the production of most splendid colours. Hand-in-hand with the development of this new chemical industry went scientific researches, which, without the use of coal-gas, could never have been undertaken. For the chemistry of the aromatic compounds coal-tar has become an, as yet, inexhausted source for new bodies, and for the artificial preparation of such as hitherto found only in the organic world.

But also the investigation of wood-tar has largely contributed to the development of organic chemistry, as shown by the short historic sketch which I have laid before you.

What a revolution, not only in chemical industry, but also in dyeing, calico-printing, and even in agriculture, the discovery of coal-tar colours has wrought, I need not dwell upon here.

Hofmann said in his reports on the Exhibition of 1862, speaking of these new colours, that England may ere long send her coal-derived blues to indigo-growing India, her tar-distilled crimsons to cochineal-producing Mexico, and her fossil substitutes for quercitron and safflower to China and Japan.

This prediction has not been quite fulfilled; but stranger events have happened. Who would then have thought that in less than twenty years the cultivation of madder would almost be a thing of the past? A few years ago a friend of mine, an eminent chemist, whose name I have mentioned to-day, was travelling in the South of France; after having seen all the sights of old Avignon, he requested his guide to take him to the madder plantations, but was told they did not grow any more. On being asked why, he replied, "Monsieur, it is now all made by machinery."

The cultivators of cochineal might almost say the same; the beautiful colour contained in it has not yet been obtained artificially, but substitutes have been found which have supplanted cochineal almost completely.

Coal-tar is not only a source of a host of most splendid colours, but also of valuable medicines—substitutes for quinine,—and the time is not far distant when quinine itself and other alkaloids will be prepared from tar. When on the Continent last year I visited one of the largest chemical works, where some new buildings were in course of erection. Asking my friend what they were for, he replied, "These are our future quinine works." What then was said as a joke may soon be said in earnest.

DISCUSSION ON THE "RIVERS POLLUTION BILL."

Sir Henry Roscoe said: As you are aware, the Council of our Society has requested the various sections to consider and discuss "The Prevention of Pollution of Rivers Bill," which has been prepared and brought into the House of Commons by Mr. Hastings, Earl Percy and Colonel Walrond, in November last, and which either in its present form or in an amended form may be brought before Parliament this session. Of the importance and at the same time the difficulty of legislating on this question, you are fully aware, and the differences of opinion which have been already expressed by various members of this Society, serve to show the obstacles which have to be overcome before the question can be satisfactorily solved. Before asking any of the members or strangers present to favour us with their views on this question, I may perhaps be allowed to state shortly the outcome of the opinions which have been so far expressed by various members of the Society in other sections.

[The Chairman then rehearsed the leading arguments of the principal speakers in the meetings of the other Sections where the subject had already been discussed. He then proceeded] :—

These abstracts give an idea of the various opinions entertained on this subject, so far as the discussion has already progressed. Of the advisability or necessity for some regular enactments there seems to be a general concensus of opinion. It also appears to be shown and felt that the application of a uniform standard to rivers throughout the country is inexpedient in the interests of the manufacturing industries of the country. Opinion is, however, much divided as to whether standards of purity of any kind should be adopted or not, and the difficulty of arriving at standards for every river, or rather for every part of every river, is too evident to need discussion. Nor is it necessary to enlarge on the essential difference between a rigid application of standards of purity in the case of the Alkali Acts, when every manufacturer is supplied with an unlimited quantity of the pure article—viz., air—and the application of similar unvarying standards to the case of water, which is supplied neither in quantity nor in quality to any manufacturers alike. The only course which then appears to be left open is to treat each case in its own merits, or to establish certain broader limits, which may be applicable to all cases. The proposal that county boards should take charge of, and have control over, the running water, whether pure or impure, in its district, appears to me to be the one most likely to lead to satisfactory results; each would be well acquainted with the necessities and possibilities of its particular county, and would apply under proper scientific advice, and with due control from a central authority as the Local Government Board, such regulations as are possible and suitable.

Alderman Walmsley, Chairman of the Rivers Conservancy of the Salford Corporation, said: I have come here to listen rather than to speak, because I want a great deal of help and guidance, and I thought I might elicit something from the discussion which would be of service to me in connection with the Borough of Salford. I have listened with a great deal of interest to the details of the Bill, as described by the Chairman, and to the results of the discussions of the various sections of your Society, and I must say that the outcome to my mind is one of almost hopeless confusion. Men of eminence in the scientific world seemed to vie with each other in contradiction, so that comparatively unscientific men, like myself, are at a loss how to deal with the practical difficulties which have to be contended with. I was very much struck with the remark of one gentleman to the effect that he would do away with all standards of purity, and require manufacturers to return the water to the streams in the same condition as they receive it. If that rule were carried out by all manufacturers up to the source of the river, they would have pure rivers so far as regards manufactories. Public authorities, in dealing with this question, had to consider two aspects of it. Firstly, they had to take care that nothing was done that would be calculated to interfere with the commercial interests too much; and, secondly, they had to consider the health of the community, and look at it in a similar manner as they did with regard to the smoke nuisance. I think anyone who has tried to deal practically with the difficulty of sewage pollution must feel very disheartened. I have at the present time an investigation going on as to the state of the Irwell from Manchester up to its source, and when I first began that investigation, I had some hope that the matter might be grappled with, but I find that the difficulty is greater and more serious than I had anticipated; still the Salford Corporation was determined to try to cope with it. With regard to the proposed Bill, it seems to me that we must not be too precise and anxious to carry out our own ideas. If we must have an Act that will be workable, it must be based on a give-and-take policy.

Dr. HEWITT: When we have to deal with rivers which have been for half a century or so the natural receptacle for the drainage of populous districts engaged in commerce, we come to a problem of great importance. If the owners of works so situated are to be deprived by legislation of the use of these natural outlets for the refuse of their manufactories, they are at least entitled to have some other outlet provided for them. No serious attempt had been made to put in operation the "Rivers Pollution Act" of 1876; that individuals had been made to suffer it was true, while others had escaped, and my belief is that it is premature to bring in another Bill for the purpose before that Act has been fairly tried. I believe that no good can be done until local influences are removed and some statesmanlike action taken for dealing with the question evenly and equally all over the kingdom. For my purpose I would prefer that the matter should be placed in the hands of officials sent down from London by the Local Government Board, because my experience of them has been that they are fairly free from prejudices; that they are always ready to hear both sides, and are exceedingly impartial. If it were left to such officials to decide what is practicable, I believe we might in a few years arrive at some satisfactory standard. For my own part I do not think that the evidence is sufficiently clear at present to furnish any definite standard. My idea is that a system of general inspection of rivers should be insisted upon, and that some such power should be given to the Local Government Board as was given to them under the Alkali Acts, in reference to noxious vapours, whereby they could require every manufacturer to adopt the best possible means of preventing the pollution of a stream. After all there can be no doubt that the greatest difficulty we have to contend with is the pollution by sewage, and I believe it is this difficulty alone that has led to so little being done to enforce the present Act. I very much admire the courageous, costly, and persistent efforts which the Salford Corporation have made to deal with their sewage. I understand that the cost incurred by the Salford Corporation is not far short of £200,000, and that their working expenses are very large. This is a very serious expense, even when dealing with the sewage of a town like Manchester; but how many towns are there which could afford such an outlay? But when all this outlay has been incurred, according to the last Report of the Commissioners on Rivers Pollution there is no known method of dealing with sewage so that the effluent from it is fit to drink, and that being so it seems to me a most cruel attempt on the part of Mr. Hastings to fix standards of purity, quite beyond any one's power to realise, for his standard of colour has been shown to be infringed by bog and peaty water. I for one would agree to any practicable standard that would put manufacturers upon one basis. If you place one manufacturer under certain restrictions which increase his working expenses, and allow another manufacturer in the same business to carry on his work in his own way, you would very likely ruin one while you assisted the other. We should have an Act of Parliament that would be entirely independent of local influences, because the present Act is often put in force against manufacturers by men who are the greatest offenders themselves. What is needed is more knowledge, a central authority to carry out the law, and that the pollution of sewage should be dealt with first. The manufacturer should have the right to turn his effluent water either into the channel he has used for years, in as pure a condition as it is possible at reasonable expense to obtain, or, at least, if this be taken from him, he should have the right to use the sewers for which he is rated, provided that he does not injure the sewers or otherwise damage his neighbours.

Mr. ARTHUR JACOB, Engineer to the Salford Corporation, said that he had looked carefully through the draft Bill, and was familiar with the provisions of the Rivers Pollution Act of 1876. It is the duty of the Salford Corporation, as the conservators of the River Irwell within their borough, to exercise a jealous supervision over the river from its source to the point where the stream passes the borough boundary. The existing Act did not, however, prove to be very workable in respect of offences committed outside the borough, for if any pollution were cast into the stream above, it was almost impossible to secure the conviction of the offender, unless the polluting matter could be actually detected in that part of the stream which passes through Salford. In consequence of this imperfection in the old Act, the river had continued to silt up, and was now in almost as bad a state as it was ten years ago. He thought it was high time that some amendment was effected in the law for preventing river pollution. No community felt this necessity more keenly than those whose avocations obliged them to remain in the vicinity of that Stygian stream, the Irwell, the state of which was a disgrace to all who suffered its condition to continue as it is at present. Dr. Hewitt had told the meeting that Salford stood forth as an honourable exception, and as an example to other sanitary authorities on the stream. The Corporation had spent more than £200,000 on sewerage works to prevent the pollution of the stream; and he (Mr. Jacob) believed that the Corporation of Bolton were also carrying out works of such a magnitude as would deal with all the sewage of that borough. It is not to be wondered at that the Irwell and Mersey are in their present foul condition, when it is recollected that besides the polluted discharges from some hundreds of manufactories, the rivers receive the whole of the refuse from a population of one-and-a-half million of people. In the existing Act it was not made at all clear what was included under the term pollution, and this made it very difficult to bring offenders to justice; but the draft Bill is much more explicit in this respect, and in other respects is an improvement on the existing law, though it will have to be amended before it can be called a thoroughly practical measure. It is too Utopian in its character, and is calculated to press somewhat heavily on the manufacturing industries; and, moreover, its language is in some parts vague and involved, instead of being plain to all, and devoid of ambiguity. With regard to the standards set forth in the proposed Bill, the discussion of them would be more for the chemist than the civil engineer. As particularised, the standards were not (as was before remarked), standards of purity; but limits of maximum impurity. Taking standard *a*, which provides that not more than one part of organic matter and three of mineral matter shall be found in 100,000 parts of water. This would prevent sanitary authorities from carrying out what is known as the duplicate system of drainage, by which all surface waters from streets, houses, courts, and yards are allowed to pass into the natural water-courses of the country. Compliance with this would involve a very burdensome restriction upon sanitary authorities, as the duplicate system usually led to considerable reduction of expense, and particularly in cases where the sewage has to be raised by steam power at the outfall. Professor Way has said "that so far as London sewage is concerned, and considering only the composition of the liquid which reaches the sewers in time of rain from the streets, it seems pretty certain that it would be as valuable in a manurial point of view as the ordinary contents of the sewers." Standard *b* does not appear to be too stringent where sewage can be treated by irrigation or downward filtration. It provides that not more than 2 parts of organic carbon, or ·3 part of organic nitrogen, shall be present in 100,000 parts of any liquid passing into a river. At Croydon, a year's analysis of the effluent water from the Norwood irrigation fields gave an average pollution of 1·515 of organic carbon, and 0·258 of organic nitrogen. This was from a pure clay soil, whilst at the Beddington farm the results were almost as good. He (the speaker) believed that these analyses had been made some years ago, and he understood that the purification was as effectual now as formerly. It seems impossible that the colour test, standard *c*, can ever be fully complied with. Rain-water passing from the surface of roads, and even from agricultural land, always retained a certain amount of colour, and not unfrequently carried a large body of grit and soil in suspension. It will be exceedingly difficult for owners of dye-works to comply with this standard, though no doubt they can effect a great improvement in the colour and quality of the water which they now send from their works.

Mr. HEYWOOD said: With regard to the standards of purity alluded to in the Bill, I should like for one moment to refer to the case of the firm I represent—viz., Messrs. Dickins & Co., of Middleton. We turn down the river Irk close upon a million gallons of refuse water per day, and at certain times it is, as Mr. Jacob said with regard to the Irwell in Manchester, as "black as ink" in consequence of our using such quantities of dyewood extracts, especialy logwood, along with the various metallic salts, etc., and in addition to these, there is of course a host of commercial drugs and chemicals as well as the different aniline dyes. What plan could we possibly adopt to purify such a volume of water so as to conform to the requirements of the Act, especially Clause 9 ? If such a case were put before the framers of the Bill, would it not convince them at once that no dyeworks or printworks of any magnitude could possibly exist under such restrictions ?

Mr. WM. THOMSON : The series of standards of pollution mentioned in the proposed Act applied indiscriminately to rivers would doubtless make highly polluted ones comparatively clear, but it would also furnish a basis for polluting pure ones. For rivers which are at present used for domestic water supplies, the standards of pollution allowed by the proposed Act are too high ; whilst for highly polluted rivers, the main advantage which would be gained would be to make the streams less discoloured. The Irwell, which I have closely examined on different occasions, and which may be taken as a typically polluted stream, is dark coloured, due mainly to solid sewage matter in suspension, which settles at parts of the river where there is little current, such as behind weirs and bends of the stream in which the water is comparatively still ; usually there is no disagreeable smell from the river, but at some points of the stream in dry and warm weather objectionable smells arise. Still on settling this water, it can be used for many delicate manufacturing processes, and it would be usually much less costly for manufacturers to settle or filter the water they require to be clear from the river than to settle their refuse waters. It may be taken as certain on the one hand that it will cost the people of Lancashire immense sums of money to keep the Irwell a little less coloured than it is at present, and the advantage gained by so doing will be little or nothing. Dr. Hewitt considered that streams in which fish live should be kept pure, because they are sources of food supplies, and I agree with him; but on the other hand to purify some streams would absorb from, instead of yielding food supply to the people, and it will give them no adequate advantage for the expenditure. I think each river is of sufficient importance to be separately dealt with, and whilst it would be advisable that some restrictions should be enforced to preserve the purity as far as possible of all streams, such restrictions should be arranged and drawn up by the people who are more or less interested in each stream.

Mr. WATSON SMITH : I have arranged my ideas, for the sake of brevity, under the following subdivisions :—

I. *(a)* Some waters, streams and rivers, must be polluted according to the very nature of the industries —*e.g.*, bleaching, dyeing, printing and the like; and in some dyeing processes, as, *e.g.*, silk preparing, bleaching and dyeing, it is absolutely necessary that water-streams as pure as possible, and that even an atmosphere as pure as possible, shall be available.

(b) In a less degree is river pollution a necessity in other industries.

(c) Hence to prevent pollution entirely, would mean destruction of some industries and the maiming of others.

(d) The fixing of impracticable standards of impurity, is tantamount to destruction of these industries—*e.g.*, the colour standard of the proposed Bill would be very hard on dyers, and it must be remembered many of their colours have enormous tinctorial power, which means that an infinitesimal weight of dye would be represented by a distinct colour.

. II. The nature of the pollutions will vary in different districts, will depend upon the prevailing industries in these districts. Thus, the Tyne will contain HCl, $CaCl_2$, alkaline and earthy sulphides, finely divided sulphur, gypsum, etc., whilst the notorious Bradford-Beck will contain colouring-matters, and dyes, fatty matters, suds, oils, besides the solutions of the various mordanting materials, etc.

III. Hence fixed standards would be unjust, and indeed absurd, for we could imagine an alkali maker having a solitary works on the banks of one river, lined with cotton mills and weaving mills, polluting wholesale, whereas a whole district of alkali works on a river like the Tyne might be laid under legislation of prohibitory stringency.

IV. There are cases, where one can conceive *pollution* voluntarily abolished, being transformed into *utilisation*, but these cases are exceptional Such transformation can usually only slowly take place.

V. To draw the line by *enforcing utilisation*, must be done intelligently and by those who know at least as much as the manufacturers about their products, and who have scientific light enough to see ahead. Even then the scheme of enforcement should be at the first, at all events, of a not too stringent or sweeping character. Then, *enforcement* would be transformed into *co-operation*—as, witness the *Alkali Act* for greatly limiting the amount of hydrochloric acid thrown into the atmosphere.

VI. To *limit* pollution and keep it within certain bounds, differing for the various districts, is possible. The limits will ever grow narrower as scientific knowledge increases, and becomes more general.

VII. No pollution of water should be allowed which communicates pollution to the air in any way, or such that the water thus contaminated will act corrosively and destructively upon materials in the river or river-bed or banks, such as brickwork, cement, mortar, stonework, woodwork and the metals lead, copper and iron.

VIII. Water-courses of every kind running into and supplying reservoirs or lakes from which drinking water is drawn, or springs serving for drinking water, ought to be strictly preserved from contamination.

IX. It ought to be remembered that sewage contamination is the most virulent form in existence, outstripping in deleterious action by far that of the factories. If all the works on the banks of the rivers Irwell and Medlock were stopped, yet their waters would still remain badly poisoned by sewage, and so be made not one whit more potable than before.

X. I agree with Mr. Coleman of the Glasgow Section, in thinking the "standards of purity" which are proposed in the Bill to be brought before Parliament, for universal application, would be utterly impracticable. But, says Mr. Coleman, there is nothing to prevent standards being established for different parts of the country, by grouping the local authorities appointed under the Bill into districts.

XI. Hence I quite think the plan supported by Sir Henry Roscoe would be best and most likely to be successful—viz., to appoint county boards, for the prevention and mitigation of river pollution, aided by scientific experts, with whom they shall consult. A standard could then be framed for each district in each county, and just and fair legislation could then be enacted, and all pollutions either prevented, mitigated, or kept within due bounds, according to the varying local circumstances. No doubt it would follow that the Local Government Board would be the centre, where the proceedings of the Provincial Local Boards would be focussed, so to say, for purposes of general reports and for legislative government action.

Dr. Hewitt moved the following resolution:—"That the Prevention of the Pollution of Rivers Bill as proposed and brought into the House of Commons by Mr. Hastings is imperfectly drawn and Utopian in character, and could not in its present shape be carried into operation without serious injury to the manufacturing and commercial community; and the Manchester Section hereby requests the Council of the Society to oppose the Bill in its present form."

Mr. E. T. Cunliffe, in seconding the resolution, whilst fully agreeing with the terms of it, said: I am of opinion in regard to our discussing this subject, that we must guard ourselves from appearing too obstructive towards any progress that may be attempted in the direction of the proposed Bill. If we take the old Act and see what it has effected, I think we are bound to admit it has done a great deal by preventing the putting into the streams of any solids. There has been no difficulty in carrying out this part of the Act, and the streams have been greatly improved by it. There can be no doubt that before any Bill can be carried out, it must be less Utopian in character than the one proposed. To frame a Bill dealing with the purification of streams beyond the exclusion of solids is too great a step at once, and I am of opinion that such standards of purity only in the first instance must be fixed which experience has shown to be practicable, and that future legislation must be progressive. I would distinguish between sewage pollution and manufacturing pollution, the central authority should deal with the former as a matter pertaining to the public health; the latter might at some future date be dealt with by county boards, who might group the polluters and appoint a commission to investigate and report as to the best means of dealing with the subject.

The resolution was then put to the meeting and carried unanimously.

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HEMPEL'S METHODS OF GAS ANALYSIS.

BY DR. W. BOTT,

The Owens College, Manchester.

It is only a comparatively short time ago that gas analysis has at all been introduced into the rank and file of the *methods of technical analysis*, but in the short space of time since then and to-day it has gained a firm and important position amongst them, and in its application to several branches of industry has become as valuable and indispensable as any of our ordinary gravimetric or volumetric processes. The great end in view of all technical methods of analysis is rapidity; at the same time this must be combined with a sufficient degree of accuracy, and a great number of processes for technical gas analysis have been devised, more or less pretending to one or both of the above requirements. The principles of all these methods, we must bear in mind, had been established once for all by the founder of systematic gas analysis, Robert Bunsen, whose methods, as far as extreme accuracy and ingenious simplicity are concerned, stand unequalled above all others. Unfortunately, however, they are scarcely applicable to technical purposes, requiring, on the one hand, a great expenditure of time, and, on the other, a considerable degree of manipulative skill, which can only be acquired by long practice and experience. Still, the majority of technical methods that have been devised, and are being devised almost every day, are but modifications of the classical methods laid down by Bunsen.

The chemical principles involved in gas analysis are only few, simple, and common to *all* methods; the difference between such methods—that is to say, the advantage of one method over another—consequently cannot be due to the employment of new re-agents, but must be sought for in the mode of applying these re-agents, and mode of measurement of the gas, or, in short, in the form of the apparatus devised for the purpose. We shall have to give the preference to such an apparatus as will admit—

1. Of an easy collection and accurate measurement of the sample.

2. Of an easy application of the respective re-agents and accurate measurements subsequently.

3. Of repeated of the same quantity of re-agent for same length of time.

Among the numerous methods proposed and apparatus constructed for technical gas analysis, those of Walther Hempel claim precedence as being accurate, rapid, and almost universally applicable. By way of qualifying this assertion, I may say that Hempel's apparatus has the following advantages:—

1. It admits of the use of all sorts of solid or liquid re-agents, including such absorbing agents as fuming

H_2SO_4, HNO_3, bromine, etc., which destroy India-rubber joints, or attack the grease used for lubricating glass stopcocks.

2. It allows of a complete exhaustion or utilisation of the re-agents, including such re-agents as pyrogallol, Cu_2Cl_2, etc., which are spoiled by contact with air.

3. It greatly obviates the errors due to the mechanical absorption of gas by the absorbing agents, and finally, it can be used for the analysis of mixtures of N, H, and CH_4, giving results which are sufficiently accurate for technical purposes.

I will now, after these few introductory remarks, endeavour to describe the various parts of Hempel's apparatus, and explain their respective uses.

First of all, we have the apparatus used for the collection and measurement of the sample of gas to be tested. This consists of a couple of gas burettes A and B (see Fig. 1), communicating with one another by an India-rubber tube. One of the burettes—the one in which the gas is collected and measured—terminates in a short capillary tube, fitted with a short piece of India-rubber tubing which can be closed by means of a pinchcock *d*. The burette B is divided into 100cc. Now, if we want to collect and measure a certain sample of gas, we proceed as follows: Both burettes A and B are completely filled with water, care being taken that all the air contained in the India-rubber tube connecting the burettes is displaced, which may readily be done by raising A, B being closed at the time, and shaking the apparatus, when any bubbles of air retained in the India-rubber tube will ascend and escape through A. As soon as the apparatus is properly filled with water, *d* is closed by means of a pinchcock, and the greater part of the water in A poured out, the connection between the two burettes being shut off by pressing the india-rubber tube while this is being done, so as to prevent any air getting into B. We now connect B by means of a suitable tube with the apparatus from which the gas is to be drawn; open *d*, and, by lowering A, syphon the gas into the measuring tube. As soon as a sufficient quantity has been collected, *d* is closed, and the apparatus allowed to stand for a minute, so as to allow the water to flow down from the sides of the tube. We then equalise the pressure in A and B by raising or else lowering A, as the case may be, till the columns of water in both tubes are exactly on a level, and then read off. It is convenient to measure out exactly 100cc. of gas, the results obtained by subsequent absorption, etc., then at once represent the percentage. In order to obviate the error due to the mechanical absorption of gases by water, it is well to employ water previously saturated with the gas to be tested. All the measurements, it is almost needless to say, must, of course, be made at temperatures as nearly constant as possible. If we are dealing with a gas containing considerable quantities of constituents, which are decomposed by, or else very

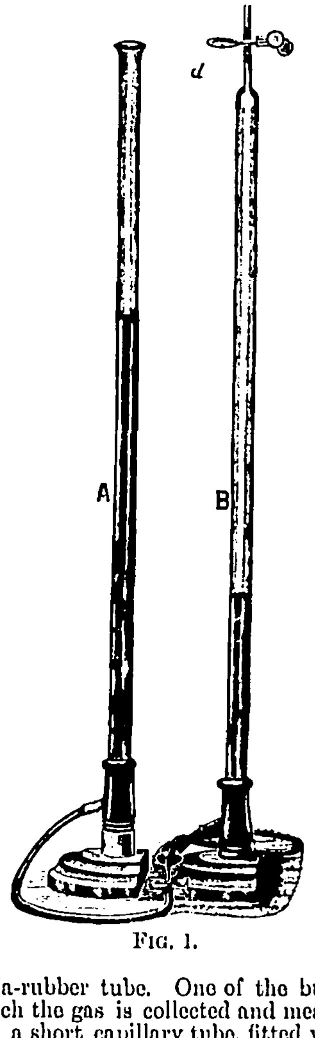

FIG. 1.

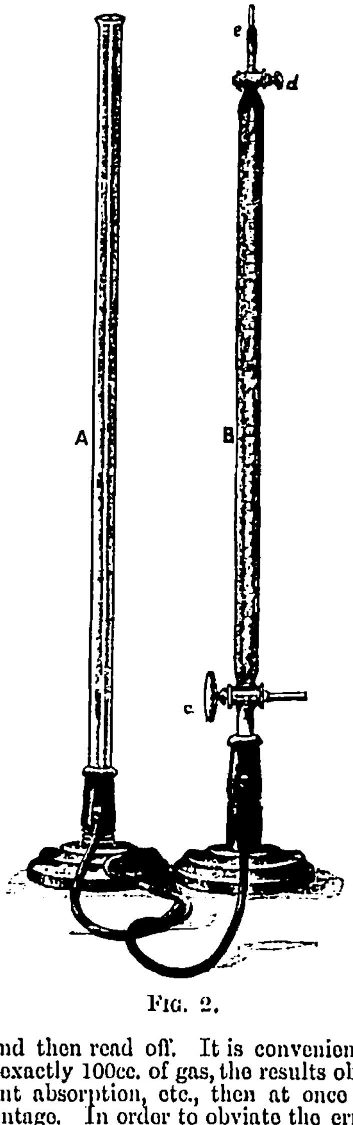

FIG. 2.

soluble in, water, the above method cannot, of course, be used. In such a case we may use another form of burette, very similar to the gas burette originally designed by Winkler (Fig. 2). It differs from the simple gas burette only by the measuring tube B being fitted with two glass stopcocks *c* and *d*: the stopcock *c* is a three-way cock, by means of which the measuring tube B can be shut off from, or set in, communication with the second tube A, or else the air outside, or a pump. The collection and measurement of samples by means of this burette is done in the following manner: The measuring tube B is thoroughly dried by drawing a current of dry air through it, communication with A being shut off by means of the three-way cock. By means of a suitable tube attached to *e*, and connected with the apparatus or space from which the gas is to be taken, the gas is drawn through the measuring tube (this is easily done by a small pump or aspirator attached to the three-way cock *c*) till all the air has been displaced by the gas, when *c* and *d* are turned, so as to shut off the gas in B from the air outside, as well as from communication with A. Water is then poured into A, and by turning *c* can be gradually admitted into B, so as to absorb the soluble constituents. When the absorption is completed, the residual volume of gas is read off, the difference between this and the original volume (100cc., since B holds exactly 100cc. when full) directly giving the percentage of soluble gases.

So much for the modes of measurements by means of Hempel's gas burette, and we have next to consider the apparatus employed for the subsequent treatment of the sample—viz., the absorption of its respective constituents. Hempel used for this purpose two apparatus, which he terms the simple and the compound absorption pipette. The simple absorption pipette (Fig. 3) essentially consists of two glass bulbs *a* and *b*, connected by means of the bent glass tube *d*. (*N.B.* The Fig. also contains a small third bulb *c*, which, however, is not strictly required; most of the simple absorption pipettes, in fact, only consist of the two bulbs *a* and *b*.) The lower bulb *b* is connected with a capillary U-tube *u*, fixed on a white porcelain plate *mm*; the whole apparatus is mounted on a suitable wooden stand. The absorbing agent is introduced into *b*, which must be completely filled with the solution, whilst the second bulb *a* remains nearly empty. A slight modification of the simple pipette is shown in Fig. 4. It is intended for the use of solid re-agents such as phosphorus, and merely differs from the ordinary burette just described, by that the second bulb *b* is made more of cylindrical shape, and has an opening at the bottom *k* communicating with an aperture *l* in the stand, through which the solid re-agent can be introduced. The apparatus shown in the Fig. represents such a pipette filled with a solution of KHO and containing plugs of wire gauze, so as to obtain a large active surface on displacing the KHO solution by the gas to be tested; it is used for the estimation of CO_2.

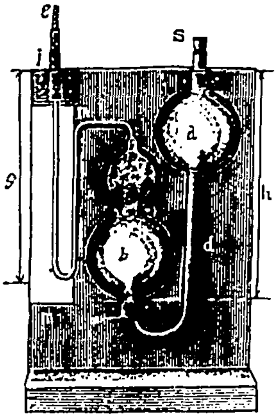

Fig. 3.

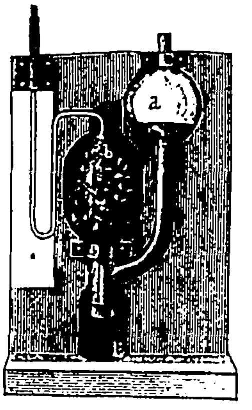

Fig. 4.

When an absorption has to be made by means of the simple absorption pipette, say with the KOH pipette represented in Fig. 5, the capillary U-tube is connected with the measuring burette containing the sample by means of a small capillary glass tube *f*, the pincock *d* opened, and by gradually raising A the gas is driven over from B into the bulb *b* of the pipette, where it displaces a portion of the absorbing fluid which rises into the empty bulb. As soon as all the gas has been transferred, the connection with B is shut off at *i* (by means of a pinchcock), and the absorption in the pipette hastened by shaking the gas with the re-agent. When the absorption is complete, the communication with B is restored, and by lowering A the gas is syphoned back into the tube B, the pinchcock *d* closed, and the volume measured in the manner already described.

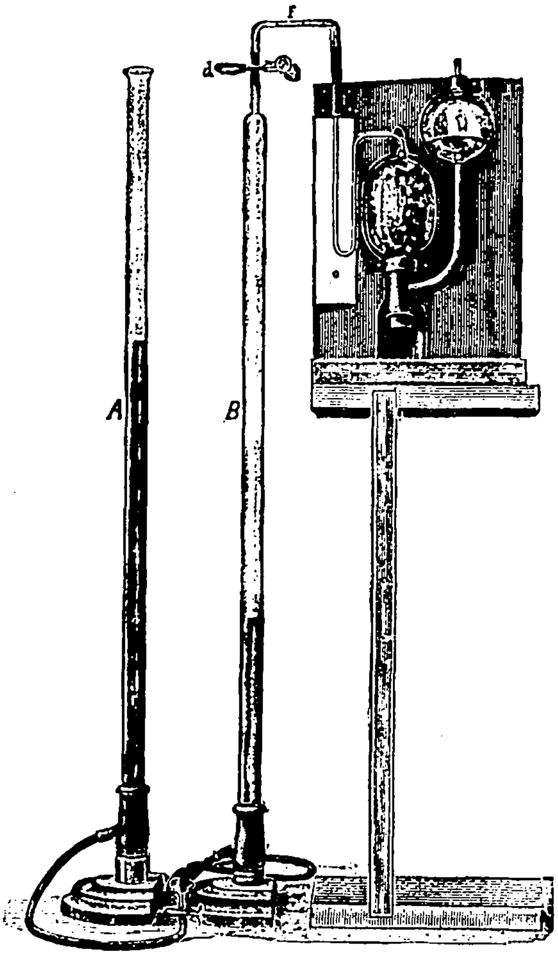

Fig. 5.

The compound gas pipette (Fig. 6) is intended for the use of re-agents which are acted upon by atmospheric air; it may be regarded as a combination of two simple pipettes, and consists of four bulbs, *a*, *b*, *c*, *d*, communicating by glass tubes. The first of these bulbs (*a*) is again completely filled with the re-agent, *f* inst., an alkaline solution of pyrogallic acid; the second (*b*) is empty; the third bulb (*c*) is filled with distilled water; and the fourth, again, is empty. When an absorption has to be made, bulb *a* is connected with the measuring burette in the manner already mentioned (see Fig. 5) and the gas passed in. The liquid displaced in *a* then rises into the second bulb *b*, and the air displaced in this is forced into the third bulb *c*, where it displaces an equal volume of the distilled water, which then rises into the fourth and last bulb *d*. Thus we see that the displaced absorbing agent in this pipette only comes into contact with the small quantity of air contained in *b*; now since the communication of *b* with the atmosphere is shut off by the water in the third bulb *c*, the action of the small quantity of air or oxygen in *b* upon the re-agent soon ceases; the oxygen is absorbed, and there remains a perfectly indifferent gas—viz., nitrogen. The compound gas pipette is used for the absorption of oxygen by alkaline pyrogallic acid solution or of CO, by an ammoniacal solution of Cu_2Cl_2 (cuprous chloride) re-agents, which must not, of course, be exposed to air—viz., oxygen.

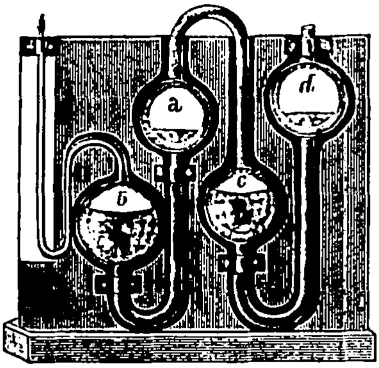

FIG. 6.

The combustion or explosion of gases is also carried out by means of a gas pipette, which is represented in Fig. 7. The bulb *b*, in which the combustion is effected, is rather thicker in the glass than the bulb of the ordinary gas pipette, and has two platinum wires fused into the top *nn*, through which an electric spark can be passed. The communication with the second bulb *a* can be shut off at pleasure by means of a glass stopcock *d*. The first bulb *b* is completely filled with a dilute solution of KHO; *a* is empty. The gas to be exploded is introduced by means of the gas burette in the usual manner, the requisite quantity of air and hydrogen added, and after closing *d* and *c*, and mixing the gases thoroughly by shaking, the mixture is exploded by passing a spark through *nn*. On opening *d* some of the liquid rises in the bulb *b* proportionally to the contraction produced by the combustion. The residual gas is then syphoned back into the burette and measured. The air required for such an explosion is of course accurately measured, and introduced by means of the burette, and so is the hydrogen. The latter is generated in a separate simple gas pipette (Fig. 8) by means of pure zinc and dilute sulphuric acid. The zinc is used in the form of a hollow stick *d*, which is fixed on a glass rod *r*, passed through a cork, and is thus introduced into the bulb *b* of the pipette, which is then completely filled with dilute H_2SO_4. The evolution of hydrogen at once commences, and *e* being closed the hydrogen evolved in bulb *b* displaces the acid, forcing it up into bulb *a*, till it is no longer in contact with the zinc, when the evolution ceases. On opening *e* the hydrogen in *b* is driven out by the pressure of the acid in *a*, which again descends into the first bulb, where it comes into contact with the zinc, and setting up a brisk evolution of hydrogen, but as soon as *e* is again closed, the acid is forced back into *a*, and the evolution stops. Thus we have in this hydrogen pipette a means of generating a continuous current of hydrogen, sufficiently pure for combustion or explosion. Regarding the chemical principles involved in the process of burning or exploding gases, and the various precautions to be observed, I must refer to the chapter in Bunsen's "Gasometry" (translated by H. E. Roscoe), as such a discussion is altogether beyond the scope of this sketch.

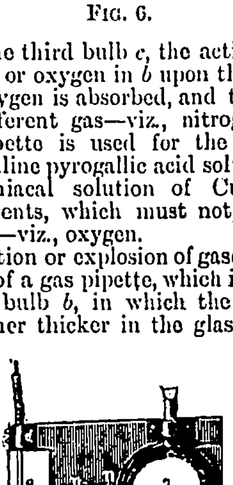

FIG. 7.

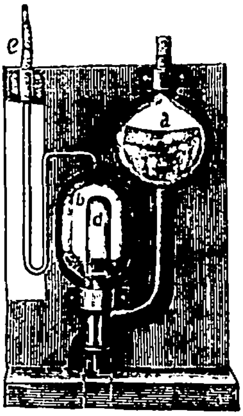

FIG. 8.

In order to illustrate the working of the whole apparatus, it will be well to consider the analysis of a mixture containing most or all of the gases likely to be met with in actual testing. Let us take a mixture of CO_2, O, CO, olefines (viz., C_2H_4, etc.), CH_4, H and N. A sample of this gas—say 100cb.m.—is collected and measured in the gas burette. We next absorb the CO_2 by passing the gas into an absorption pipette, containing a solution of 1 part of KHO in 2 parts of water (Fig. 4). To ensure a rapid and complete absorption the bulb *b* containing the caustic potash is partly filled with plugs of wire gauze, which on displacement of the potash solution by the gas offer a large surface moistened with the re-agent. The absorption of the CO_2 is almost instantaneous—all we have to do is to pass the gas into the apparatus and syphon it back again to be measured. The contraction produced directly gives us the percentage of CO_2, since we started with 100cc. The remaining gas contains O, CO, H, C_2H_4, CH_4, N; we next absorb the oxygen. This may be effected in two ways—by means of moist phosphorus or by *an alkaline solution* of pyrogallic acid. The former method is by far the more elegant of the two, but not universally applicable. The absorption is done in an ordinary pipette (Fig. 9), the bulb (*b*) of which is filled with thin sticks of yellow phosphorus surrounded by water. The gas to be tested is introduced in the usual manner, and by displacing the water in *b* comes into contact with the moist surface of the phosphorus which speedily absorbs all the oxygen from it. The absorption proceeds best at about 15-20° C., and is complete in ten minutes. The small quantity of phosphorus acid formed by the absorption dissolves in the water present, and thus the surface of the phosphorus always remains bright and active. As has already been mentioned, this neat and accurate method is not universally applicable; the following are the conditions under which it can be used:—The percentage of oxygen in the gas must not be more than 50%, and the gas must be free from ammonia, C_2H_4 and other hydrocarbons, vapour of alcohol, ether and essential oils. In the instance we have chosen, the phosphorus method would hence not be applicable, as our mixture contains C_2H_4. We must therefore resort to the second method using pyrogallic acid. The absorption is carried out in the compound absorption pipette (Fig. 6), the bulb *b* of which *is completely filled* with an alkaline solution of pyrogallol made by dissolving 1 part (by volume) of a 25 per cent. pyrogallic acid solution in 6 parts of a 60 per cent. solution of caustic potash. The absorption is complete in about five minutes, but may be hastened by shaking. The remainder C_2H_4.CO, CH_4, H, N of our gas now contains N, and the next step is to absorb the C_2H_4, which is done by means of fuming, and determine the CH_4 by explosion. Let us choose the latter method and take a portion, say a-half, of our residual gas for the estimation of hydrogen. The absorption of the hydrogen is based on the fact that palladium in a finely divided state—so-called palladium black—is capable of completely burning hydrogen when mixed with excess of air, and slowly passed over the metal at the ordinary temperature. About 1½grm. of palladium black are placed in a small U-tube (Fig. A) plunged into cold water, and the gas mixed with an excess of air (which of course must be accurately measured) is passed slowly through the tube two or three times, the tube at the time being connected with an ordinary absorption pipette filled with water or else with the KOH pipette, which in this case, of course, simply serves as a kind of receiver. Finally the gas is syphoned back into the burette and measured—two-thirds of the contraction correspond to the amount of H originally present in the mixture of gas and air. The CH_4 is not attacked by sulphuric acid (ordinary 30 per cent. SO_3 Nordhausen acid answers well enough). The acid is contained in an ordinary absorption pipette (Fig. 10), the small bulb *c* of which is filled with *pieces of broken glass or glass* wool, so as to offer a larger absorbing surface to the gas. The absorption is complete in a few minutes, but the remaining gas previous to measuring should be passed into the KOH pipette and back again, so as to free it from fumes of SO_3. Residual gas: CO, CH_4, H, N. The carbonic oxide is next absorbed by means of an ammoniacal solution of cuprous chloride in a compound absorption pipette. The gas has to be shaken with the absorbent for about three minutes. It must be borne in mind that Cu_2Cl_2 solution also absorbs oxygen, and, according to Hempel, considerable quantities of C_2H_4, hence these gases must be removed previously. Residue: CH_4, H, N. We may now either determine both CH_4 and H by exploding with an excess of air in the *explosion pipette and measuring* (1) the contraction produced, and (2) the amount of CO_2 formed (by means of the KOH pipette); or we may, according to Hempel, absorb the hydrogen first of all in this reaction—provided the U-tube be kept well cooled with water. At about 200° C, however, a mixture of air and CH_4 is also acted upon by palladium. The *presence of CO*, vapours of alcohol, benzene and hydrochloric acid interferes with the absorption by palladium.

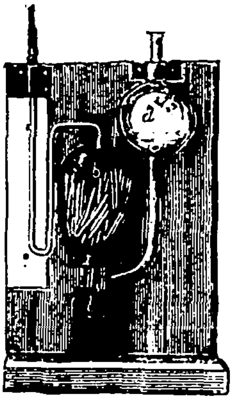

Fig. 9.

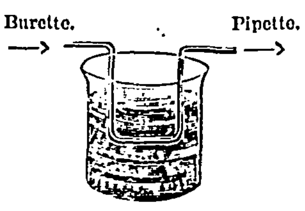

Fig. A.

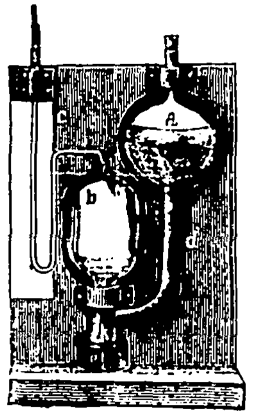

Fig. 10.

The palladium can be used for many consecutive experiments, but must be kept as dry as possible. After it has been used for several absorptions it may be regenerated by plunging the tube into hot water and passing a current of dry air through it.

Having estimated the hydrogen, we have lastly to determine the CH_4 in the remainder of our gas. This contains CH_4N and H, the amount of the latter being known *from the previous experiment*. The gas is mixed with the requisite quantity of air and hydrogen, introduced into the explosion pipette and fired by means of a spark. The water resulting from the combustion condenses in the bulb of the pipette, whilst the CO_2 formed is absorbed by the KOH solution present. Hence the total contraction produced corresponds to:

a. The hydrogen present in the original gas + ½ its vol. of O (the quantity requisite for comple combustion).

b. The known quantity of hydrogen added + ½ its vol. of O.

c. The CH_4 present+2 vols. of O requisite for its combustion.

$$\underbrace{\underbrace{CH_4}_{2} + \underbrace{O_4}_{4}}_{6} = \underbrace{(CO_2 + 2H_2O)}_{\text{disappears.}}$$

Since *a* and *b* are known, or can be readily calculated from the previous data, we can by subtracting $(a+b)$ from the total contraction obtain $C-(a+b)=c$ contraction due to CH_4 alone, and one-third of this is equal to the volume of CH_4 present, as will be readily seen from the above equation.

The remaining nitrogen is estimated by difference, a direct method of determination not being known at the present time.

The above brief sketch will at all events suffice to show the value of Hempel's methods for technical purposes. A complete analysis of a complex mixture like the one chosen for the purpose of illustrating these methods need only occupy twenty to thirty minutes, so that a great number of such analyses may be carried out in a day. Moreover, with due care and skill, the results obtained are very accurate indeed; in fact, more accurate than is generally required or expected from a technical process.

DISCUSSION.

Dr. Hewitt: We are greatly indebted to Dr. Bott for his valuable paper. All I can say is, if we have not thoroughly grasped all he wished to convey in his demonstrations with the apparatus, he has nevertheless reduced a very complex system of testing gas to a very simple operation, and I certainly think that syphoning back must give a degree of accuracy which is not otherwise attainable. We know that there are many processes which require continual testing for their elucidation. It is not merely necessary to know what may be done in the course of 24 hours, but it is absolutely necessary to know what is going on from time to time. I am sure that Dr. Bott's method would be valuable in making the tests of gases in the working of Deacon's process for the manufacture of chlorine. Difficulties often arise in the analysis of mixed gases, and I should like to have had the opportunity of having the mixed gases in Deacon's process determined by some simple method like the one before us. I have no doubt, from what I have seen to-night, that possibly this apparatus might be of considerable assistance in facilitating the rapid determination of the carbonic acid and hydrochloric acid which abnormally are found in the gases passing to the bleaching powder chambers, and which, when present in any appreciable quantity, defeat the manufacture of good bleaching powder.

Dr. Markel: I have had some experience with the apparatus before us, and would therefore like to add, as regards its accuracy, that on comparing it with Bunsen's method I found the results obtained agreed within 0·1 to 0·3 per cent. for each gas. The gas analysed on this occasion contained CO_2, O, Co, H, CH_4, N. Besides showing this surprising degree of accuracy, Hempel's method is the most expeditious of any I know. I have made as many as fourteen analyses daily with it. Dr. Bott has remarked that it is necessary to allow the gas to stand for a few minutes in contact with the first reagent. I do not think it is necessary to let the gas remain longer in contact with the potash than is required to pass it into the pipette and back again into the burette, when the CO_2 is completely absorbed. The O is absorbed by the alkaline pyrogallol in two to three minutes. Phosphorus is preferable for absorbing the oxygen, but as the above-mentioned gas contained traces of ammonia, it could not be used. Co is also completely absorbed in the pipette fitted with ammoniacal cuprous chloride in two to three minutes, provided the pipette is well shaken. The palladium-black causes unition of the H and O, when the gas is allowed only to pass once over it and back again. It can be used for an indefinite number of times, provided the water which collects in the tube is removed from time to time by allowing warm air to pass over it. The whole operation ought not to exceed half an hour if everything is well prepared.

Liverpool Section.

Chairman: E. K. Muspratt.
Vice-Chairman: J. Campbell Brown.
Committee:

Ernest V. Bibby.
Hudson A. Binney.
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C. Symes.
G. I. J. Wells.
F. Hurter.
H. Brunner.
A. Norman Tate.
J. Affleck.

Local Sec.: E. G. Ballard, Queen's Park, St. Helens.

Notices of papers and communications for the meetings to be sent to the Local Secretary.

The next meeting of this Section will be held at University College, Ashton Street, April 1, 1885, when the following will be the order of proceedings:—(1) Election of Officers for Session 1885-86. (2) Discussion on Mr. Pratt's Paper, read March 1. (3) Paper on Electric Lighting, by Mr. Holmes.

University College, Ashton Street, Liverpool,
Wednesday, March 4, 1885.

MR. E. K. MUSPRATT IN THE CHAIR.

"THE RIVERS POLLUTION BILL."

BY E. K. MUSPRATT.

As one of the most useful functions of our Society is to take into consideration, and to criticise with the knowledge of experts, any proposed legislation of the character of the "Rivers Pollution Bill," I have at the request of the Committee prepared a paper on the subject.

I must, however, take this early opportunity of saying that I am by no means competent to deal in a comprehensive manner with this most difficult subject, and that I have undertaken the task solely with the view of raising a discussion on the Bill by members more competent than myself.

The growth of manufacturing industry, together with its manifest blessings and the consequent increase of the well-being of the people, is unfortunately accompanied by evils which it is our duty, and I hope our desire, to mitigate. The framers of this Bill, I am afraid, either from ignorance or with design, attempt by law absolutely to prevent them. From my reading of the Bill there is no clause similar to the clause in the Act of 1876, which exempted the manufacturer from the penalties of the Act, if he used the best practicable means for rendering harmless the polluting liquid which he sent into a stream. It follows, therefore, if this Bill becomes law, that unless he can purify the liquids flowing from his works, so as to satisfy the standards prescribed by the Bill, a manufacturer must absolutely close his works, and sacrifice all the capital invested therein. As all of you have read the proceedings of the London Section, in which Mr. Cresswell gave a very able exposition of the Bill, it will be unnecessary for me to attempt to call your attention at any length to the legal aspects of the question. Mr. Cresswell says the Act of 1876 was "a piece of lame and impotent legislation," and this Bill is brought in to remedy its defects. Now, the prime defect of that Act, he says, is the want of machinery for carrying it into execution, and he points to the reluctance of the sanitary authorities (the greatest culprits themselves) to put the Act into execution, but admits that recourse may be had to the Local Government Board, to compel the sanitary authorities to put the Act in force.

He then complains of the delay caused by the visit of the inspector, who really has to determine upon the best practicable means to prevent the pollution. This, then, is the grievance, and this Act is intended to remove it, which it proposes to do very effectually by prohibiting the running of any polluting liquid into any stream or river, whether tidal or otherwise, irrespective of cost to the miner or manufacturer.

I think, however, the case against the Act of 1876 has not been made out. Mr. Cresswell says fifty-three proceedings have been taken under the Act, and *twenty-four orders* to restrain pollution have been obtained, but have never been enforced at all.

Now, why have these orders never been enforced? I applied to Mr. Cresswell for an explanation, and he wrote to me to say, "that it is not the duty of the Local Government Board to enforce them; on the contrary, they have in almost every case intervened to extend the period assigned for compliance with the order under Clause 3 of the Act of 1876, and that, *e.g.*, the order in the Hereford case, which was made in 1878, has been suspended in successive years until February, 1885." But by this Clause 3 the Local Government Board can only grant this extension of time if they are satisfied, after local inquiry, that further time ought to be granted to enable the sanitary authorities to adopt the best practicable and available means for rendering harmless the sewage matter complained of.

Now as Mr. Cresswell admits that in some cases the Act has been put in force, it seems to me that in this Hereford case it is probable no available means are at hand, although, as we are aware, there are several towns which, to a great extent, do render their sewage harmless, it seems strange that nothing has been found practicable—in this special case—for seven consecutive years.

In the case of manufacturing pollutions, the difficulty may be greater, and the cost to the individual manufacturer may be so great and so much out of proportion to the necessity of the case, that the Local Government Board would be fully justified in stopping proceedings on the ground that "some material injury will be inflicted by such proceedings in the interests of such industry."

But is it not possible, without altering the intention of the Act, to improve its machinery? We are all agreed that pollution of rivers should be prevented, provided the cost of such prevention is not too great, and if the present Act is as faulty as it is said to be, something should be done to amend it.

If we attempt to march too fast we may very easily frustrate the object we have in view, and it is desirable, in the first instance, to ascertain what is practicable in the way of prevention. We know how the Royal Commissioners, at the instigation of Dr. Frankland, decided that certain standards of purity were attainable, and these standards are practically adopted in the Bill we are considering.

It was a fatal error that on the Commission only theoretical chemistry was represented in the person of Dr. Frankland, for I need hardly remind members of our Society that something beyond a knowledge of scientific chemistry is required to decide what is practicable in chemical operations on a large scale. But even scientific chemists are by no means unanimous in favour of the standards recommended.

Mr. Crookes is of opinion they are on an entirely erroneous basis, and I think we must all agree with him that to lay down absolute standards of purity for every river was a mistake.

It certainly seems more reasonable that each river should be considered on its merits. The addition of certain substances which by themselves might pollute a pure stream, would be beneficial in the case of the Irwell or the Mersey, and until sewage matter is kept out of the Thames the addition of salts of iron, manganese, zinc, or bleaching liquor, for example, need not be prohibited. It is straining at a gnat and swallowing a camel if, before we have dealt effectually with the great sewage question, we press on chemical manufacturers enormous expense in order to improve the quality of the liquids escaping from their works.

If the germ theory of the origin of cholera and typhoid be true, it is far more important in a river like the Thames, which is a source of potable water, to prevent its pollution by sewage than by manufacturers' refuse, and yet, so far as I know, this has not yet been accomplished.

It is true Sir John Lawes seems of opinion that the sewage of London itself is of great benefit in increasing the catch of fish in the Channel, but the inhabitants of London ought not to be obliged to drink the sewage of the population above the intake of the water companies, and if pollution of this character can be prevented there would be no reason against enforcing the adoption of similar means of purification in towns on other streams. Whether the standards recommended by Dr. Frankland are attainable or not, some standard for sewage pollution might certainly be found after practical experience in dealing with the sewage on a large scale, and such standards might fairly be made more stringent as the methods of purification improved. What is really wanted is a practical method of dealing with sewage on a large scale, and the experience of the last ten years ought to be able to furnish us with one.

What might fairly be required of an inland town ought not to be required for a town on an estuary like the Mersey, and the same principle—*i.e.*, having regard to the general character of the river—should be applicable to pollution from manufacturing processes. I am therefore of opinion that Mr. Crookes is clearly on the right path in making the river or stream, and the uses to which it is to be applied, the standard of pollution, and not the liquid running into it.

The criticisms of the manufacturers when the standards of the Pollution of Rivers Commission were first published are in the main applicable to those in the present Bill.

[Mr. Muspratt here read extracts from the evidence of Mr. Stevenson and others before the Royal Commission.]

As regards solid matter, Mr. Newlands pointed out at the London meeting that the great bulk of the rivers of the kingdom contained more than the standard as suspended mud, and that the colour standard in subsection C was far too stringent, and really is of no use, for many natural rivers or streams are much more coloured.

Mr. Johnson complained in London that in some cases they were too stringent and in others too lax, particularly those referring to acidity and alkalinity, if they were permitted in any stream employed subsequently as a water supply.

In many cases these standards are absolutely impracticable, and Lord Salisbury, in introducing the Bill of 1875, admirably summed up the objections to them: "The difficulty that was felt with regard to these tests was that they struck too far, and that on the one hand they would include water flowing from perfectly innocent sources, while on the other hand they were not sufficiently stringent to affect the very nuisances against which the legislation was directed."

After giving this Bill my best consideration, I have come to the conclusion that it is framed on an entirely wrong principle, inasmuch as it proposes the same standards of pollution irrespective of the locality and circumstances of the case.

If passed into law, it would have a most injurious effect on the industry of the country, as many works and mines could not possibly comply with its provisions. If the present Act cannot be enforced on account of the power given to the Local Government Board to supervise or restrain its application, the attempt to give this power to individuals, or even to induce individuals to complain of pollution by paying the penalties for the offence to them, as it is suggested in Clause 3, might be grossly abused for private purposes.

The power to set the Act in motion ought to rest with a public body, and if the sanitary authorities are interested parties, the new County Boards, which are sure to be established at an early date, seem to be the proper body for the conservancy of the rivers in their district, and the preservation of the rivers from pollution.

The attempt to fix standards of universal application is wrong in principle, but after some experience local standards on the lines of Mr. Crookes' suggestion—viz., that the river itself should be the standard of purity, and that no liquid should be allowed to be sent into a river if the liquid contains a greater percentage of impurity than the river itself—might possibly be adopted.

I have in the foregoing pointed out how injurious to manufacturing interests this Bill might become, if made law, but have not attempted to go into any minute details on the chemistry of the question, which I know will be ably handled by other members present.

Dr. Hurter has informed me of some investigations in Saxony which bear out the view I have taken, that until the sewage question is dealt with, the keeping-out of manufacturing refuse, except solid matter, which may interfere with the flow of a stream, is hardly necessary.

Metallic salts disappear from solution in a marvellous manner, and cannot be traced at even a short distance from the place where they flow into a stream. These same investigations will also support the view that regard must be had to the condition of the river itself, both as to its velocity and the volume of water; and I have no doubt the discussion this evening will throw further light upon many of the points I have so lightly touched upon.

The subject has now been discussed in most of our sections, and the result will be to give the Council of the Society ample information to guide them in any suggestions they may have to make to those in authority, on this Bill or any other that may be brought in to deal with this most important question.

DISCUSSION.

Dr. **Hewitt** thought it was clear upon the evidence before the Rivers Pollution Commission, that sewage pollution and trades pollution stood upon a totally different footing. The sanitary objection had arisen not from trades pollution, but from sewage pollution. He thought it would be a new feature in English law if simply for æsthetic reasons Parliament were to interfere with those industries by which England had gained her greatness. The greater evil undoubtedly was sewage pollution, and in many instances it interfered seriously with the health of towns. That this was a very difficult question no one would deny, especially when he heard the enormous cost to Salford of their sewage system. While the sanitary authorities are the chief sinners, they are constituted the authority to carry out the law and put pressure upon the manufacturers, but they had found practical difficulties in dealing with the question. The chairman of the Conservancy Committee in Salford acknowledged that the more he tackled the question the more the mountain of difficulty seemed to grow, and he found that it was not a question to be gone into lightly, or that very strict standards could be employed. Dr. Hewitt held that something should be done, but whatever was done, it should be carried out in a uniform manner. The only point upon which he (Dr. Hewitt) was inclined to differ from their Chairman was as to the estuaries. There was a great difference in estuaries, and some distinction should be made between one estuary and another. They must not introduce a system which would bear more hardly upon one manufacturer than another. They must not press upon the one higher up the river, and leave the one lower down to escape altogether. If so, this would introduce between men in the same trade a very objectionable want of uniformity. He would support almost any degree of strictness in a Noxious Vapours Bill, because it was required from all manufacturers in the same trade to conform to the same standard. It was abundantly clear that sewage effluents could never be fit for potable water, and, therefore, there was no need to seek for such a standard of purity from the manufacturer; it was idle to expect that the standards in this Bill could be realised, for natural sources of pollution would often make the river to infringe such standards. If they passed an Act of Parliament which could not be realised, there would be absolutely no results from it, whereas if they could pass an Act of Parliament which could be carried out by some conservancy board or county board, working under the Local Government Board, some good might be effected. If a river was an important source of food supply, then it was very important to take steps to prevent its serious pollution, but the case was different when the rivers were used for drainage and trade purposes alone. The Mersey and the Irwell had ceased entirely to be sources of food, and to insist upon the same standard in reference to these rivers, as for those other rivers to which he had referred, would, in his mind, be to inflict a lasting injury upon trade in which the interest of this country was so largely bound up. He therefore thought the Legislature would hesitate to pass such a Bill. It had been proposed that instead of sending the waste water from manufactories into the rivers it should be sent into the sewers. The men who owned large works were those who paid the large rates, and nobody had a better right to use the sewers than those who so largely paid for them, and if this user of the sewers caused any damage the manufacturer should be made liable for it. At the same time he admitted some little pressure should be brought to bear on the manufacturers, as they were quite ready to blame each other and do nothing. Any one reading the replies to the inquiries addressed by the Rivers Pollution Commissioners could see that each manufacturer was impressed with the value of good influx water, and was ready to apportion its value in money, but was very silent about the effluent, or held that nothing could be done. An Act might be passed which placed rivers under inspection, and required "the best practicable means" to be adopted, and if such a measure was put in force uniformly, fairly, and gradually all over the kingdom, due regard being had to difference in the value of rivers as sources of potable water, food supply, etc., then some very considerable improvement might in time be brought about.

Professor J. **Campbell Brown** said: In this matter there were two opposing interests; on the one side there was the interest of the manufacturer and the public who used his products. Every one would agree that our manufactures ought not to be needlessly hampered; and most people would agree that they must not be altogether stopped, even if some of our rivers had to be spoilt, and they ought not to forget the interest of future manufacturers, although that might have little weight compared with that of those already established. On the other side there was the interest of the riparian owners, the riparian tenant-farmers, and all those various interests that were represented by conservancy boards, including the people who fish in the streams, whether for food or for sport. The object of legislation ought to be to give the maximum of freedom to each side, with the minimum of interference with the other. This being the case, he did not think he could altogether agree with Mr. Muspratt, when he said that no legislation ought to interfere with the impurities cast into the stream by manufacturers until after sewage was dealt with, because although no doubt large quantities of sewage impurities cast into a stream were very prejudicial to health in every way, still the introduction of moderate quantities of sewage did not interfere with the fish, did not interfere with the farmer, did not injure his cattle, although it rendered the river useless for domestic purposes. But the introduction of effluent water from some factories would destroy the river for any useful purposes, except for carrying certain refuse materials into the sea. For instance, paper makers could keep out of the rivers nearly the whole of the poisonous esparto resin, and they might also keep out the largest portion of the fibrous matter, and after recovering most of their alkali, they might neutralise the remainder by some of the acid which they used for bleaching purposes, and then the effluent water would not destroy the fish. He thought every reasonable man would agree that the paper makers, for instance, ought to be compelled to bring their effluent water up to that standard of purity. In some other cases he thought it was very likely that legislative powers could not be put in force with any degree of stringency, without great hardship. He did not see any other way out of the difficulty than by agreeing that there should be different standards for different streams. There might be some streams devoted to manufactures, and others devoted to the riparian owners and the tenants. He entirely agreed with Mr. Muspratt in all he said with regard to standards, and he did not think that they could fix standards which should be applicable to all rivers, and which it would be just or convenient to make compulsory through the whole country. It was utterly impossible, and he did not think it was at all necessary or desirable. If standards could be agreed upon, they would no doubt greatly assist the inspector; they would define his work, and by a chemical analysis he would be able to prove his case easily in a court of law. But an Act of Parliament should be for the benefit not of the inspector only but of the whole nation. Every stream should be treated on its own merits, taking into consideration its condition before the effluent water passed into it, and the use made of the river below, and also the importance of the manufacture, and the possibility of purifying the effluent water at a reasonable cost. If it did cost the manufacturer some money he ought to be called upon to bear it for the good of the public. Where it would be ruinous to an important manufacture to purify the effluent water, such manufacture should be allowed to flourish only on certain rivers which were not reserved for fish and for other purposes.

Mr. Sinclair pointed out that the Bill had been brought in by three private members and not by the Government, and he thought it would be utterly impossible to get the Bill into a shape that would be acceptable, and it ought to be opposed until it was preceded by a General River Conservancy Bill brought in on the responsibility of the Government. Looking at his notes on the old Bill of 1873, he found that some of the most objectionable tests which were struck out of that Bill by the Committee of the House of Lords, to whom it was referred, were reintroduced in the present Bill. When the rivers pollution question was dealt with they should take care that no proceedings were taken except with the sanction of the Local Government Board, and that nothing should be required from the manufacturer that was not reasonably practicable, and that he should not be subject to prosecution when he kept within the prescribed tests. The Local Government Board should make more use of the power they had to make local sanitary authorities do their duty. He made this remark, because the Local Government Board did not exercise the power they now had in their hands.

Mr. Henry Brunner could not agree with that clause in the Bill by which any aggrieved individual could compel the sanitary authority to take proceedings. He thought the authority which made the law ought to have the power of enforcing it and the responsibility for its being carried out. At present if a man could prove injury, he had his remedy at common law, but it was the cost of obtaining that remedy that deterred people, and thus rivers continued to be polluted. Under the Bill as proposed, a great amount of vexatious litigation would arise. He doubted whether county boards would form a proper authority for dealing with this matter, and should prefer one central authority. He thought the question of fixed standards extremely objectionable, and not likely to attain the desired object, as they would enable either sanitary authorities or manufacturers, so long as they were within the standard, to turn into a pure stream matter which would nevertheless pollute it, and although some of our rivers were little better than common sewers, it was desirable that some at least of the others should be kept as pure as possible. Though he was of opinion that pollution by sewage matter most of all required consideration and attention, he did not think that the prevention of river pollution was specially directed against the manufacturing interests of the nation. If the law said from the beginning that no sanitary authority or manufacturer should be allowed to turn into a stream matters of worse character or in greater quantity than the water of the stream already contained, and that the state of the stream itself should be the standard of purity for that stream, he believed that such a law would assist in the development rather than restrict the progress of manufacturing industry. But in Lancashire—probably elsewhere also—they had had instances where beautiful streams of pure water had been purposely polluted by manufacturers with the object of preventing their use by possible competitors. He thought that the true interests of manufacturers lay in preventing the pollution of rivers as far as possible, and certainly no individual or collection of individuals should have the right of so polluting a stream, whether for their own profit or convenience, that no one else could use it after them.

Dr. Hurter thought that the Bill met some of the objections raised by Dr. Hewitt by Clauses 7 and 8, which provided that existing rights should not be affected by the provisions of the Bill, but the exact meaning of those clauses could only be made out by lawyers. As regards the standards of purity, Professor Mills, when discussing them at Glasgow, considered some of them not sufficiently stringent to preserve the life of fish. Now, it appeared to him (Dr. Hurter) that it was not required that fish should be able to live in the fluids discharged from works into the rivers, but only in the mixture of the polluting fluid with the river water. Dr. Hugo Fleck, an eminent German chemist, had lately published a small pamphlet on the pollution of rivers in Saxony, and from this little book Dr. Hurter gathered Dr. Fleck's opinion on the subject. Dr. Fleck thinks that we cannot expect anything from river water but that it shall be sufficiently clean for washing purposes. He considers a river as the natural drain of the river valley. Dr. Fleck thinks that in determining the permissible pollution regard must be paid to the amount of water passing through the cross section of the river and also to the velocity of the stream. He thinks it would be no use to forbid the industrial establishments to pollute a river so long as the much larger pollution from town sewage is not excluded, because the pollution from sewage is usually much the most serious. In many instances it was found that the polluting matter from factories reacted upon the polluting sewage matters so as to purify the river. Dr. Fleck sums up with the following words:—"If we regard the rivers in the first place as the natural drains of the valleys, then we shall have to renounce the maintenance of a large stock of fish in a river in all cases where a large population, with numerous industrial establishments, inhabit the river valley. No rules would ever be able to keep out of a river everything that might prove a disturbance to fish life, where a river was at the same time in the service of both domestic and industrial life. Very often the effluvia of industrial establishments were represented as the cause of the decrease of fish, without any regard to the fact that the sewage of dwelling-houses might often have its influences. It was proved that the water-worms (*naïdes*), who were the greatest enemies of roe, were found in those deposits of mud which were rich in fæcal matters. Wherever a river was in the service of manufactories it would be impossible to make rules of Parliament which would re-establish in the river a large stock of fish." Thus Dr. Fleck's opinion goes a long way towards making each river its own standard—*i.e.*, towards the suggestion of the Chairman.

Mr. Muspratt, replying on the discussion, said he did not mean that county boards alone should be the authority, but what he meant was that if conservancy boards were appointed for different rivers, or drainage areas, the county boards should be represented on the conservancy boards. He agreed with Dr. Hewitt in the Local Government Board having supervising power. They must have a central authority to see that all interests throughout the country were equitably and properly dealt with. He also agreed that it was absolutely impossible to fix standards at present, but it might be possible after they had had a lengthened experience, and after investigation by the new boards, who might take the whole question into consideration. They would have to consider the interest of each locality, and the best practical means of dealing with the pollutions in that locality. Dr. Campbell Brown scarcely agreed with him that the sewage should be first dealt with; but after all, sewage was the greatest source of pollution, and even in an æsthetic point of view, they should have regard for the nose as well as the eye. He certainly thought more progress might have been made if proper inspectors had been appointed under the Act. They wanted to find out the best practical means of dealing with the sewage, having regard to the reasonable cost. He thought the present Act gave ample power for dealing with pollutions. It insisted upon the best practical means being adopted for the prevention of pollution. He thought they were all agreed that this should be done. They did not want to continue the pollution, and he thought all that Dr. Brown said with regard to paper makers was perfectly just and right. Paper makers were like everyone else—bound to use the best possible means to prevent pollution. He thought the intention of the Act was strong enough, and it was only a question of carrying out that intention. He therefore proposed that that meeting of the Liverpool Section of the Society of Chemical Industry "was of opinion that the Rivers Pollution Bill of 1884 was inadequate to secure the objects of its promoters, and would be injurious to the interests of manufactures, and recommends its early consideration by the Council of the Society, with a view to such action as the interests of industrial chemistry might require."

Mr. Carey seconded the motion, and it was carried unanimously.

THE LOSS OF ALKALI OCCASIONED BY SULPHUROUS ACID IN THE LEBLANC PROCESS.

BY J. W. PRATT, F.C.S.

The question indicated by the title of my paper relates to one of the last stages of the manufacture of alkali by the Leblanc method.

The sulphate of soda produced in the earlier stages of the manufacture has already been decomposed, and the resulting black-ash lixiviated to yield the soda in solution before the loss which I have attempted to trace is set up. Once the alkali is obtained in solution as vat liquor, it is only necessary, in order to procure the various solid soda ashes, etc., to subject it to evaporation and concentration. In English works the concentration of the vat liquor is usually effected by causing it to flow from the vessels in which it has been prepared and settled, into long open pans attached to the rear of the revolving or hand black-ash furnaces, where the flame and residual heat play freely over its surface.

As the evaporation progresses, monohydrated carbonate of soda or "salts" separates out and a mother, or "red liquor," remains. The salts are removed by fishing, and the "red liquor" is also run off from time to time.

Further manipulation converts the "salts" into the purest form of commercial soda or carbonated ash, and the "red liquor," in which the original impurities of the vat liquor are concentrated, into caustic ash. But the initial concentration which leads to this subdivision of products, and to so large an amount of water being evaporated economically, is not without its attendant disadvantages. One of these is the introduction of foreign matters into the alkali through the agency of the fire gases, which consist essentially of carbonic acid and oxide, unburnt oxygen, and invariably a small proportion of sulphurous acid, due to the sulphur present in the fuel employed. It is with the action of this last gas, sulphurous acid, that my experiments have to deal. The other gases, excepting the action of the CO_2 on the causticity of the liquor, are inert or at any rate harmless. But sulphurous acid being easily absorbable by the alkaline solution, leads to the formation of sulphites or hyposulphites subsequently oxidised to sulphate, and the alkali thus neutralised is to all intents and purposes lost. This loss of alkali has been variously estimated. Weldon* has estimated it at about 2 per cent., and Moorhouse[*] at from 0·10 per cent. to 0·20 per cent. sulphur on the soda-ash made.

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* Journ. Soc. Chem. Ind., January, 1883.

The figures given below, on which my estimate is based, are the results of experiments carried out at two independent works of the Runcorn Soap and Alkali Co., Limited, with the kind permission of the general manager, Mr. Charles Wigg. During two periods, some months apart, I had taken for me at each works average weekly samples of the coal used for firing the furnaces, and of the vat and red liquors, salts, carbonated and caustic ashes produced.

The workmen took small portions of each of the substances daily for a week, and these weekly average samples were then divided down and analysed. After analysis, the weight of the substances being known, and their ratio one to another having been previously calculated over long periods of production, all the required data were to hand.

The total alkali was estimated in each sample, likewise the sulphide, hyposulphite, and total sulphate after oxidation with hypochlorite, acidification and precipitation as barium sulphate. All the results are calculated on 100 of Na_2O, after deduction of the Na_2O existing as sulphide. The total iodine consumed in the titration of the solutions, after deduction of that due to the sulphide, is reported as hyposulphite, according to the method usually practised in alkali works laboratories. I have so reported it in order to avoid complication and possible error through the employment of not very reliable methods for the determination of sulphite, hyposulphite, sulphide, sulphate and total sulphate when existing together in one solution. This arrangement can only affect the relative amounts of sulphate of soda mechanically and chemically introduced in the preliminary experiments.

The analysis which follows is the average of several months, and will serve to show the general composition of the vat liquors dealt with :—

Na_2O = 100

$NaHO$ = 26·75%
Na_2S = 2·87
$Na_2S_2O_3$ = 1·02
Na_2SO_4 = ·58
Total Na_2So_4 after oxidation = 6·81
$NaCl$ = 10·16
$Na_4Fe(CN)_6$ = ·313
Na_2SiO_3 = 2·68
$Na_2Al_2O_4$ = 1·50
Iron = 0·213[†]

The fuel employed was almost entirely derived from one source, and hence does not present much divergence in composition. So far as the present subject is concerned, the only point of particular interest is the amount of volatile sulphur it contains, as that of course leads to the formation of the deleterious sulphurous acid in the fire gases. Its analysis gives :—

Fixed carbon = 57·50%
Volatile matter = 29·25 = volatile sulphur 1·7%
Ash = 8·25
Moisture = 5·0

100·00%

A. WORKS, OCTOBER, 1881.—No. 1 EXPERIMENT—INITIAL EVAPORATION.

The pan attached to the revolver in this works measures 28 × 8 × 1·5 feet, and contains 336 cubic feet liquor, exposing a surface of 224 square feet.

"Salts" made = about 60 tons per week.

Total Na_2O dealt with—about 50 tons per week.

120 firing coal to 100 Na_2O.

Ratio Na_2O as "salts" to Na_2O as red liquor—1·5 : 1.

	Vat Liquor.	Red Liquor.	Salts.
Na_2S =	1·89	5·45	nil.
$Na_2S_2O_3$ =	1·52	5·60	·57
Total Na_2SO_4	7·12	23·80	2·32

$$\frac{(23\cdot8\times40)+(2\cdot32\times60)}{100}-7\cdot12=3\cdot79 \text{ increase of}$$

sulphate of soda on 100 of Na_2O. This figure will of course include the sulphate of soda mechanically carried over by the draught of the furnace through the revolver. The hyposulphite calculated on the above basis

$$\frac{(5\cdot6\times40)+(0\cdot57\times60)}{100}-1\cdot52=1\cdot06\%\ Na_2S_2O_3$$

in 100 of Alkali. These results give the alkali neutralised and lost by sulphurous acid as being ·41% Na_2O, and the increase in total sulphate

Mechanically carried over = 1·89%
Due to increase in $Na_2S_2O_3$ = 1·90

Increase in total Na_2SO_4 = 3·79%

A. WORKS.—No. 2 EXPERIMENT—JANUARY 11, 1885.

	Vat.	Red.	Salts.
Na_2S =	2·526	5·57	nil.
$Na_2S_2O_3$ =	1·310	4·13	0·8
Total Na_2SO_4 =	7·90	21·13	2·57

$$\frac{(21\cdot43\times40)+(2\cdot57\times60)}{100}-7\cdot90=2\cdot21\% \text{ increase}$$

Increase in hypo = 0·95% = 0·372% Na_2O lost

Na_2SO_4 mechanically carried over = ·51%
Na_2SO_4 due to increase in $Na_2S_2O_3$ = 1·70

Total Na_2So_4 = 2·21%

As the "salts" are removed and roasted into carbonated ash, and the red liquors are evaporated to form caustic ash, in furnaces where top heat is in each case employed, a further exposure to sulphurous fire gas is involved before the alkali is finished. The figures below show the extent of this loss :—

A. WORKS.—100 Na_2O.

	Salts to Furnace.	Finished Carbonated Ash.
Na_2S =	·013%	nil.
$Na_2S_2O_3$ =	·930	·375
Total Na_2SO_4 =	1·170	1·900

1·9 − 1·47 = ·43% increase of sulphate or a loss of ·187% Na_2O on 100 of alkali.

	Red Liquor.	Caustic Ash 48%
Na_2S	2·117	·22
$Na_2S_2O_3$	5·605	(Na_2So_3) 6·32
Total Na_2SO_4	20·813	21·61

·827% increase of Na_2So_4 = ·360% Na_2O lost.

Taking the mean of the two first experiments, and proportioning the amounts of alkali lost in finishing the two ashes, we find the total loss of alkali and increase in sulphate of soda on 100 of Na_2O to be thus arranged :—

		Na_2O lost.
Na_2SO_4 mechanically introduced =	1·20% =	—
" chemically introduced =	1·80 =	·391%
" carb. ash (1·5) furnaces =	·258 =	·112
" caustic ash (1·0) furnaces =	·331 =	·111
Total introduced =	3·589% =	·617%

This finishes the experiments at Works A, and the figures which follow are those obtained at the much larger works of the two.

B. WORKS.—NOVEMBER, 1881.

The samples are average ones from pans attached to two revolvers.

The dimensions of the pans are 46′ × 8′ × 2′ and 45′ × 6′7½″ × 2′, and they expose a joint area of 666 square feet to the flame, with a cubic capacity of 1332 cubic feet.

Salts made—about 170 tons per week.

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[*] Trans. Newcastle Chem. Soc., 1871.

[†] Of this iron, 27% exists as sodium ferrocyanide, and 73% as ferroso-sodium sulphide, $Na_2Fe_2S_4$ (H_2O).

Total Na_2O dealt with—about 130 tons per week.
Coal—104 : 100 Na_2O.
Ratio Na_2O as salts to Na_2O as red liquor—1·5 : 1·0.

No. 1 EXPERIMENT.—NOVEMBER, 1884.

	Vat Liquor.	Red Liquor.	Salts.
Na_2S	= 1·36	3·121	nil.
$Na_2S_2O_3$	= 1·296	5·350	·311
Na_2SO_4	= 6·00	19·02	2·318

Calculated as before :—

Na_2SO_4 mechanically carried over = 1·11%
,, due to $Na_2S_2O_3$ acid = 1·88

Total increase = 2·99%
Loss of Na_2O = ·412%

No. 2 EXPERIMENT.—JANUARY, 1885.

	Vat Liquor.	Red Liquor.	Salts.
Na_2S	·575	1·268	nil.
$Na_2S_2O_3$	1·718	6·91	0·417
Na_2SO_4	6·816	23·150	2·111

This experiment gives :—

Na_2So_4 mechanically carried over = 1·192
,, due to increase in $Na_2S_2O_3$ = 2·40

Total increase = 3·892
Loss of Na_2O = ·521%

B. WORKS.—FURNACING OF SALTS.

Coal to 100 Na_2O as Carb. ash = 0·9560 : 1.

	Salts.	Finished Carbonated Ash.
Na_2S	nil.	nil.
$Na_2S_2O_3$	·311	·282
Na_2SO_4	2·3185	2·775

2·775 – 2·3185 = ·46% increase of sulphate.
Loss of Na_2O = ·2%

B. WORKS.—RED LIQUOR TO CAUSTIC ASH.

Ratio coal to 100 Na_2O as caustic ash = 2·18 : 1·0.

Na_2S	3·121		0·173
$Na_2S_2O_3$	5·350	Na_2SO_3	6·740
Na_2SO_4	19·020		20·382

Increase of sulphate = 1·362%
Loss of Na_2O = 0·6%

Taking the mean of the two pan experiments, and proportioning the increase of sulphates in the two ashes, we find the total amount of sulphate capable of being introduced—from beginning to end of the operations—to be thus distributed on 100 of Na_2O :—

		Na_2 lost.
Na_2SO_4 mechanically carried over	= 1·301%	= —
,, due to hyposulphites	= 2·110	= ·468%
,, introduced in furnacing	= ·82	= ·36
Total introduced	= 4·261%	= ·828%

It will now be noticed that these figures place the amount of total sulphate introduced about five times higher than the estimate of Moorhouse, and approximate more closely to that of Weldon. Of course it will be readily apparent that differences of conditions and environment may account to a large extent for the divergence. As regards the proportion of the total sulphur in the fuel employed absorbed by the alkali in course of manufacture, the ratios which I have established in the various stages as subsisting between fuel and Na_2O dealt with, afford the necessary data. In A Works, the proportion of sulphur absorbed from beginning to end of the operations is 7·42%.

In B Works the percentage is 10·1%.

Analysis of the revolver gases for sulphurous acid before entering and after leaving the salting-down pan place the proportion rather higher.

To sum up, my figures, when averaged, would point to the following conclusions, which I have drawn up in a tabulated form on 100 Na_2O :—

	Alkali lost.	Sulphate introduced.
Salting-down pan	·43%	3·22%
Carbonated ash (1·5) furnaces	·116	·267
Caustic ash (1·0) furnaces	·192	·41
	·738%	3·027%

Finally, it might be of interest to the members if those who have worked with pans fired from underneath would state in how far the extra fuel employed in that method compensates for the above and other disadvantages of the overhead evaporation of alkaline liquors.

Newcastle Section.

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John Morrison.
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H. R. Procter.
W. W. Proctor.
J. F. Stark.
T. W. Stuart.

Local Secretary and Treasurer: J. T. Dunn, 115, Scotswood Road, Newcastle.

Notices of papers and communications for the meetings to be sent to the Local Secretary.

Next Meeting, April 23rd.—"On Insecurity of Written and Printed Documents," by Mr. J. Jameson. "Some Account of Recent Progress with the Jameson Coking Process," by Mr. J. Jameson. "On some Practical Results of the work done by Messrs. Black and Larkin's Patent Salt Cake Furnace at the St. Bede Chemical Company's Works, East Jarrow," by Mr. W. Rennoldson.

Meeting held 5th March, 1885, in the Lecture Theatre of the Durham College of Science.

MR. B. S. PROCTOR IN THE CHAIR.

ON THE PROPOSED STATUE TO NICOLAS LEBLANC; ON THE RECOVERY OF SULPHUR FROM ALKALI WASTE; ON A FURNACE FOR CHEMICAL OPERATIONS WHICH IS NEITHER AN OPEN FURNACE NOR A CLOSE FURNACE; AND ON THE MANUFACTURE OF CHLORINE FROM OXYCHLORIDE OF MAGNESIUM.

BY WALTER WELDON, F.R.S.

In the Address which I had the honour to deliver in this theatre in July last, I spoke as though a statue had been recently erected in France in memory of the author of the most beneficent chemical invention which has yet been made by man, Nicolas Leblanc. No such statue has, however, been erected; and I am here this evening mainly for the purpose of correcting, on the spot at which it was committed, the error on this point into which I then fell.

Within the preceding eighteen months, I had read, not in one journal only, but in many journals, that a certain municipality had decided to erect a statue to Leblanc. I had read in the *Comptes Rendus*, that eloquent address to the Académie des Sciences, by its then Perpetual Secretary,—not only the greatest chemist, but nearly if not quite the greatest orator of his time: Jean Baptiste Dumas, now, alas! no longer among us,—which commences: "La municipalité d'Issoudun a résolu de consacrer par un monument la mémoire d'un glorieux enfant de cette cité, le créateur de l'industrie de la soude artificielle." And I shared, not only with many of my own countrymen, but with many Frenchmen, the belief that this resolution had been carried into effect.

The statement founded upon this belief which I made here in July has been reported in the Journal of our Society. It ought therefore to be corrected therein. Except, however, abstracts of matter published elsewhere, nothing can appear in our Journal which has not first been communicated, either to the Society at large, or to one of its Sections; and hence the main motive of the communication which I have sought permission to make to its Newcastle Section this evening.

The project of erecting at Issoudun a monument to Leblanc dates from 1856: the year of the publication of that famous "Rapport relatif à la Découverte de la Soude Artificielle," drawn up by Dumas, on behalf of a Committee of the Academy of Sciences consisting of Dumas, Thenard, Chevreul, Pelouze, Regnault, and Balard: which establishes so conclusively that Leblanc really was the author, and the sole author, of the process which bears his name, and that the attempt of one of his old partners, Dizé, to claim the credit of that great invention was dishonest and unfounded. It was then believed that Issoudun was Leblanc's birthplace; and the leading Literary Society of the ancient province of Berry, of one division of which Issoudun is the capital, initiated the idea of erecting there a monument to the man most useful to his fellows that that province could claim to have produced. Only local subscriptions were sought; few were obtained; and the project had to be abandoned. It was revived in 1877, and again in 1881; in each case with the same result as in 1856. At length, in 1883, it was adopted by the "Consultative Chamber of Arts and Manufactures" of Issoudun, with the concurrence of the Municipal Chamber of that city; and for some months there seemed every prospect that this time it would certainly be realised. Only for some months, however; for in November, 1883, the first-mentioned of these two Chambers came to a decision with respect to it which shows that at Issoudun local patriotism is confined within most curiously narrow bounds.

One of the most remarkable figures in the intellectual society of Paris of to-day, is that of a distinguished painter, who for the last fourteen years has been afflicted with blindness — M. Auguste Anastasi. Humble in his estimation of the talent which enabled him to produce, before his great affliction fell upon him, works so esteemed by others that they have provided adequately for his later years, this chivalrous blind artist is proud indeed of one thing—namely, that he is a grandson of Nicolas Leblanc. In 1883, soon after the Chamber of Arts and Manufactures of Issoudun had decided to accomplish the erection in that town of a monument in memory of his illustrious ancestor, there came into the possession of this gentleman some family papers, which led him to doubt the accuracy of the belief which at that time obtained with respect to the date of his grandfather's birth; and the researches which he at once set on foot with a view to clearing up this point resulted in the discovery, not only that Leblanc was born in 1742, and not in 1753, as had been believed for nearly three-quarters of a century, but also that he was born, not at Issoudun, but at a village near Issoudun — namely, Ivoy-le-Pré. M. Anastasi at once communicated this last-mentioned discovery to the Chamber of Arts and Manufactures of Issoudun: and the members of that body regarded it as showing that a statue to Leblanc in their unquestionably ancient but apparently not very enlightened little town "had no longer any *raison d'être.*" Upon the principle of which conclusion, that statue of George Stephenson which one sees from the door of this building would have been in place at Wylam, but is out of place in the capital city of the district in which Wylam is situated.

The work which was thus abandoned by the Issoudunites is now, however, in course of accomplishment; and our own country, which is certainly the one which has profited most by the invention of the art of obtaining soda from salt, has an opportunity of taking part in it. It is now in the hands of a committee in which France is represented by nearly all her leading chemists, including Berthelot, Boussingault, Becquerel, Cahours, Debray, Frémy, Friedel, Péligot, and Schlœsing, by eminent men who are not specially chemists, including Pasteur, De Freycinet, and the Count de Lesseps, and by all her largest chemical manufacturers; and in which Germany is represented by Professor Hofmann of Berlin, Belgium by M. Solvay, and this country by three Vice-Presidents of our Society, Sir Henry Roscoe, Colonel Gamble, and myself. France has already subscribed liberally; thanks to M. Solvay, Belgium holds a prominent place on the subscription list; under the auspices of Professor Hofmann and Mr. Hasenclever, influential committees are being formed for the collection of subscriptions in Germany and Austria; and the three English members of the central committee have undertaken to bring the matter under the notice of every soda maker in Great Britain. The present is certainly not a propitious moment at which to ask British Leblanc soda makers to write cheques; and I am not concerned to try to maintain that to erect monuments to departed benefactors is our highest duty. But it happens to be just now that the opportunity presents itself of doing for Leblanc what was long ago done in this country for James Watt,—I put it in that way, because I think that Dumas was certainly right in regarding the Leblanc soda process as the invention of the last century next in practical importance to the steam engine; it would at least be unbecoming that we, who manufacture twice as much Leblanc soda as all the rest of the world put together, should be surpassed by any other nation in our willingness to contribute to the acknowledgment now in question of the indebtedness of all mankind to an invention which exceeds in practical importance all others of modern times, one only excepted; and other countries are doing their part in the matter so well, that the total sum which one need wish to raise in this country in respect of it is so small,—say some £600 or £700 or so,—that if only every English soda maker would contribute something, the amount required from each would be quite inconsiderable.

Before passing to other matters, I would ask permission to mention that M. Anastasi has recently written a biography of Leblanc,* which was published at Paris last summer, and which I believe that Messrs. Hachette & Co., of 18, King William Street, Strand, London, would send by post to any one in Great Britain in exchange for two shillings. Though its details are necessarily scanty, it tells the story of Leblanc's career much more fully than that story had been told before; and a more deeply pathetic history of a human life is scarcely, I think, to be found in all literature. Of the new facts stated in it, I will leave to be gained from the book itself all but just one: from which it appears that in travelling to Paris by Calais or Boulogne one probably traverses, and if one does not actually traverse, passes within a stone's throw of, the spot where black-ash was first manufactured, and the spot on which Leblanc died by his own hand. It is usually stated that his death took place in a hospital for the poor, but it really took place in his own house; that house was within the walls of his works; and upon a portion of the site of his works now stands the railway station of St. Denis.

Having thus corrected the inaccurate statement made by myself in this theatre last July, I will ask permission to proceed to refer to certain comments on the process proposed by Dr. von Miller and Herr Opl for the recovery of sulphur from alkali waste, which occur in the very interesting paper by Professor Divers which was read before this Section at its meeting in November. Of the value and importance of Dr. Divers's work on the chemical history of calcium sulphides and hydrosulphides, it would be difficult to speak too highly; but he has been misled—evidently in large measure by imperfect information with respect to Von Miller and Opl's methods of procedure—into basing on some of the results of that work quite erroneous conclusions with respect to a process, one part of which might, I think, be turned to account in this district.

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* "Nicolas Leblanc; sa Vie, ses Travaux, et l'Histoire de la Soude Artificielle." Par. Aug. Anastasi, ancien artiste peintre, Chevalier de la Legion d'*Honneur*, petit-fils de Nicolas Leblanc. Paris: Hachette.

After having pointed out two very real objections to an earlier proposal, by Herr Opl only, for causing the calcium sulphide of alkali waste to enter into solution as calcium sulphydrate, and having added that "a third objection is that calcium sulphide is too slow for technical purposes in combining with hydrogen sulphide, according to our experiments on the small scale," and that "a fourth difficulty is that the calcium hydroxide, and some, at least, of the carbonate always present in alkali waste, have also to be saturated with hydrogen sulphide," Dr. Divers goes on to say: "Opl's later process, or, rather, Opl and Von Miller's process, is equally unpromising. A solution of calcium hydrosulphide having been prepared from alkali waste, this is to be made, by boiling, to give up its hydrogen sulphide. Part of this is to be converted into sulphur or sulphuric acid at pleasure, and the rest is to be used in preparing a fresh solution of calcium hydrosulphide by dissolving out with it in water the sulphide of alkali waste. Not only are there the objections which apply to the other process, that hydrogen sulphide by no means quickly and readily dissolves calcium sulphide, and that it has to saturate the free lime in the waste, but there are obviously others. A solution of calcium hydrosulphide only slowly decomposes, even when vigorously boiled, unless the atmosphere over it is rapidly changed and the liberated hydrogen sulphide in this way carried off; and, therefore, the expulsion of the last portions of hydrogen sulphide will involve the generation of much steam, and this steam has to be condensed and the gas cooled before it comes to the moist waste. Another difficulty to be met, is to force this gas into the closed vessel holding the waste, since without pressure it could never be passed with any speed into the waste and yet be all absorbed."

Now, to deal first with those of these supposed objections to the process of Messrs. von Miller and Opl which relate to that portion of it which consists in treating waste by H_2S, in order to bring the calcium sulphide of the waste into solution as calcium sulphydrate, this part of that process has been in industrial operation for now nearly two years, and experience has shown that it is open to Dr. Divers's criticism of it only on one point. The operation is not performed in "a closed vessel;" it involves no "forcing," or pumping, of sulphuretted hydrogen, either "under pressure" or otherwise; the sulphuretted hydrogen is absorbed as rapidly as it can be practically supplied; and although that sulphuretted hydrogen does react on any calcium hydrate contained in the waste treated by it, if it attacks at all the calcium carbonate of the waste, the extent to which it does so is quite inappreciable.

With pyrites at threepence per unit, it is obvious that a process directed solely to the recovery of sulphur from alkali waste, and yielding that sulphur only as sulphuretted hydrogen, can have little chance of adoption; and hence, at least until there shall have been realised some very cheap method of treating H_2S for the separation of its sulphur in the free state, those of the proposals of Dr. von Miller and Mr. Opl which are criticised by Dr. Divers in the passages which I have quoted, taken by themselves, would have little or no practical interest. It is only because one of those proposals has been proved applicable with advantage as part of a process, having a certain interest for this district, of which the recovery of sulphur from alkali waste is simply a secondary result, that I take this opportunity of submitting to you certain facts relating to it with which Dr. Divers, when he wrote his paper read here in November, was obviously unacquainted.

While it was at the Alkali Works of Hrüschau, in Moravia, of which Dr. von Miller is one of the proprietors, Mr. Opl being chemist there, that the treatment by H_2S of a mixture of waste and water, so as to dissolve out the calcium sulphide of the waste as calcium sulphydrate, was first put into execution: as soon as this method of getting the sulphur of waste into solution was found to be a really practicable one, it was put aside at Hrüschau, pending the result of that work in respect of methods for the obtainment of the sulphur of calcium sulphydrate as free sulphur which Dr. von Miller and Mr. Opl are still earnestly pursuing. Almost simultaneously, however, with its proposal by these chemists, simply as a step towards the recovery of sulphur, it was proposed also, by M. Lombard, of Marseilles, as part of a process for the manufacture of "dicalcic phosphate;" and that nearly two years' experience of it to which I have referred has taken place, not at Hrüschau, but, in connection with this process of M. Lombard's, at Rassuen, in the south of France.

The old method of manufacturing "dicalcic phosphate" consists in dissolving a suitable phosphatic material in aqueous hydrochloric acid, and then precipitating the phosphoric acid of the resulting solution by means of finely divided calcium carbonate. The phosphate obtained in this way, however, is never pure, by reason of some of the calcium carbonate always getting so coated with insoluble phosphate that contact between it and the solution in which it is present cannot take place. Seeking to become able to obtain a richer precipitated phosphate than had as yet been obtained industrially, and seeing that he could hope to do this only by means of a precipitant which could be employed in the state of solution, M. Lombard was at length led to the idea of employing as the required precipitant solution of calcium sulphydrate, obtained by dissolving the calcium sulphide of alkali waste in aqueous solution of sulphuretted hydrogen. The process in which he does this was got to work at Rassuen, on a small but industrial scale, early in 1883; it has been in operation there ever since; and a much larger than the original plant for it is now being constructed there.

Of the sulphuretted hydrogen which is liberated in the first operation of M. Lombard's process, by the reaction upon each other of solution of phosphoric acid and solution of calcium sulphydrate, half of course corresponds to the sulphuretted hydrogen which was used as such in the preparation of that solution of calcium sulphydrate, and must be employed for the preparation of another quantity of solution of that body, only the remaining half of it being available for the separation of free sulphur from it, or for use, as at Rassuen, as a source of SO_2. The H_2S, however, is not, as Dr. Divers supposed when he wrote the paper read here in November, divided initially into two portions: one to be sent directly to burners, and only the other into the apparatus in which it is to react on waste; but the whole of the sulphuretted hydrogen is sent, not into a "closed vessel," containing merely "moist waste," but into the first of a series of three vessels, charged with a mixture of waste with more than its weight of water: the first of which three vessels communicates with the second, the second communicating with the third, the third communicating with apparatus for the combustion of H_2S into SO_2 and vapour of water, and this last communicating with vitriol chambers. Each of the three absorption vessels is furnished interiorly with a mechanical agitator, which is kept in motion throughout the operation performed in them. The whole of the sulphuretted hydrogen enters, as I have said, the first of the three vessels; a portion of it is absorbed in that vessel; and the remainder of it passes on into the second vessel. Of the H_2S which thus enters the second vessel, a portion is absorbed therein, the remaining portion passing on into the third vessel; and from the third vessel that portion of the sulphuretted hydrogen entering it which is not absorbed in it passes on to the combustion apparatus. The H_2S has not to be forced into the absorption vessels, but is drawn into them: partly by the draught occasioned by the absorption of some of it in those vessels, and partly by draught from the combustion apparatus and the vitriol chambers. The necessary division of the total quantity of H_2S into the proportion of it which may be burned, being the proportion of it, per operation, which represents the quantity of sulphur recovered from waste in that operation, and the proportion of it which may not be burned, and which may be regarded as one and the same quantity of H_2S, kept constantly in circulation, and used continually over and over again for dissolving calcium sulphide out of successive quantities of waste, is effected in the easiest possible way, simply by duly adjusting to each other the total quantity of H_2S evolved in each operation and the quantity of waste charged into the absorption vessels.

In M. Lombard's process,—and it is only in connection with some such process as his that there can as yet be any question of the preparation of calcium sulphydrate from alkali waste,—the only influence of the presence of calcium hydrate in the waste treated is to diminish the quantity of sulphur recovered from waste, in the state of sulphuretted hydrogen, obtained as a secondary product of the process, per given quantity of the phosphate which is its primary product. When there is no calcium hydrate in the waste employed, one ton of sulphur from waste is obtained, as sulphuretted hydrogen, per somewhere about three tons of dicalcic phosphate; and seeing that, at the present price of pyrites, a ton of pure sulphuretted hydrogen, for use merely as raw material of the *manufacture of sulphuric acid, cannot be regarded* as worth more than thirty shillings, for the quantity of secondary product, per that quantity of primary product, to be reduced, by the presence of calcium hydrate in the waste employed, to say eighteen hundredweight, is simply to make the cost of the primary product greater in the latter case than in the former by about one shilling per ton. As regards the calcium carbonate of the waste employed, it has practically no prejudicial influence whatever, sulphuretted hydrogen not attacking this constituent of waste until practically all its calcium sulphide and calcium hydrate have been dissolved out, beyond which point the operation is not carried; and though Dr. Divers supposes, I believe erroneously, that sulphuretted hydrogen generated from solution of calcium sulphydrate by Dr. von Miller's method, "must be cooled before" it can be used for the preparation *of a further portion of sulphydrate, in M. Lombard's* process, at any rate, the H_2S does not require cooling, being evolved at a temperature little, if at all, above that of the atmosphere. Lastly, the operation of bringing into solution as calcium sulphydrate the calcium sulphide of alkali waste, by the method which I have described, is not a costly operation: requiring fuel only for raising the small quantity of steam required to keep in motion the agitators in the absorption vessels, and labour only for raising that steam, charging those vessels, and discharging them.

We may, therefore, I think, take it that calcium sulphydrate is now readily and cheaply obtainable for use in any process, to be performed in or in the *neighbourhood of Leblanc soda* works, in which that body can be employed as a re-agent: provided only that the process be one—such, for example, as M. Lombard's—in which the sulphur of the calcium sulphydrate, or, if not the whole of that sulphur, at least half of it, is evolved as H_2S. For what cannot, I think, fail to become a very important industry in this district—namely, the manufacture of phosphates of lime from the phosphoric acid of the slag of the basic steel process, the question of using as the necessary precipitating agent solution of calcium sulphydrate prepared by the method of Messrs. von Miller, Opl, and Lombard, and so obtaining a richer phosphatic product that can be obtained by means of any other precipitant industrially available, and at the same time recovering the sulphur of an equivalent or nearly equivalent quantity of the calcium sulphide of alkali waste—certainly seems worthy *of at least being studied.*

As Dr. von Miller's method of decomposing the solution of calcium sulphydrate obtained by his, Opl, and Lombard's method can have very little practical interest until means shall have been realised of cheaply obtaining free sulphur from sulphuretted hydrogen, I will ask permission to devote to it only the very few sentences necessary to indicate its real nature—for the idea of it which is conveyed by Dr. Divers's paper is not an entirely accurate one—and its present position. Dr. Divers's statement that "solution of calcium hydrosulphide decomposes only slowly, even when vigorously boiled, unless the atmosphere over it is rapidly changed, and the liberated hydrogen sulphide in this way carried off," is of course absolutely accurate; but it has scarcely any relevance to Dr. von Miller's method of procedure, which consists, not in merely "boiling" the solution in question, but in evaporating it nearly to dryness. This method of procedure no doubt involves "the generation of much steam," and "this steam," as Dr. Divers says, "must be condensed;" but the condensation of steam is not a difficult operation, and the only practical question relating to that steam is, what is the cost of raising it? On *this point* Dr. von Miller informs me that such experience as he has yet had with respect to it leads him to conclude that the evaporation of the quantity of solution of calcium sulphydrate, the evaporation of which yields the quantity of sulphuretted hydrogen containing two tons of sulphur, being the quantity of such solution which in his process has to be evaporated per one ton of sulphur recovered from alkali waste, can be effected by 1·8 ton of slack, and that in his district, at any rate, the value, merely for use in agriculture, of the 2 tons 6 cwt. of chemically pure but very dense crystallised calcium hydrate which are obtained as a secondary product of the evaporation of the quantity of solution of calcium sulphydrate in question, is fully equal to the cost of performing that evaporation, for fuel and labour combined. The one difficulty which he has really had to encounter in his endeavour to realise on *the large scale the decomposition of* calcium sulphydrate by the mere evaporation of its aqueous solution is that of providing suitable vessels in which to perform that evaporation: vessels, the materials of which shall be attackable neither by calcium sulphydrate nor by hydrogen sulphide. He believes that he has recently come to see a satisfactory way out of this difficulty; but at this season building operations are impracticable at Hrüschau, and it will not be till towards the close of next autumn that the practicability or otherwise of his latest ideas on this subject will have been adequately tested.

Dr. von Miller is perfectly conscious that even when he shall have succeeded in obtaining command of suitable apparatus in which to effect the decomposition of calcium sulphydrate by his method, that method can have little practical value until it shall have been supplemented by a cheap means of decomposing sulphuretted hydrogen in such manner as to obtain its sulphur in the free state. Though hydrogen sulphide is by no means a very stable body, none of the many proposed such methods of decomposing it have yet succeeded on the large scale: even the Claus process, which at one time seemed very hopeful, having as yet given only incomplete results. Dr. von Miller is at present working on an idea for the accomplishment of the object in question, which, whether or not it be destined to prove more fortunate than its many predecessors, is at least interesting as constituting quite a new departure in its line: that idea being to burn the hydrogen only of sulphuretted hydrogen, not by gaseous oxygen, but by the solid oxygen of calcium sulphate.

Dr. von Miller has found that if sulphuretted hydrogen be passed over heated calcium sulphate, the calcium sulphate becomes reduced to sulphide, and there passes off a mixture of vapour of water and vapour of free sulphur. He has also found that if a current of air be then admitted to the residual calcium sulphide, the latter combines with some of the oxygen of the air thus admitted to it, with considerable evolution of heat, the original quantity of calcium sulphate being thus reconstituted for use again. The oxygen which combines with the hydrogen of the sulphuretted hydrogen treated by this process is thus atmospheric oxygen, but atmospheric oxygen no longer having nitrogen associated with it; so that if any of the sulphuretted hydrogen escapes decomposition in the first instance, or if any of its sulphur is oxidised into SO_2, the H_2S or SO_2, which in such case passes from the apparatus in which the process is performed, instead of being mixed, as under corresponding circumstances in the Claus process, for example, with so much inert gas as to preclude its utilisation, is mixed only with vapour of sulphur and vapour of water, and when these have been condensed out of it remains perfectly pure. At Oldbury, Messrs. Chance Brothers have made some small experiments on this process; and they have found that, in the laboratory at any rate, it is easy of execution, and that none of the sulphur of the sulphuretted hydrogen treated by it undergoes oxidation, provided only that the sulphuretted hydrogen be always sent into the apparatus containing the heated calcium sulphate at such speed that a slight proportion of it shall pass out undecomposed.

Other new processes for the recovery of sulphur from the residual product of the Leblanc soda process are now under experiment, but time will not permit me to touch upon them; and I will now proceed, with your permission, to briefly indicate the nature of the furnace invented at Salindres for use in the process of manufacturing chlorine from oxychloride of magnesium.

That method of manufacturing chlorine was first made public by the address, delivered in this theatre, to which I have already referred. Shortly afterwards, one of your members, who bears a name which must always hold a foremost place in the history of metallurgical invention, Mr. Hugh Lee Pattinson, junior, wrote to that great daily newspaper which is one of the many things for which this city is renowned far and wide, for the purpose of pointing out certain supposed objections to that process, in order, as he put it, "that an undue amount of importance" might "not be attached to" the address in which that process and certain probable consequences of it were touched upon, "by those unable, of their own knowledge, to supply a criticism" of it. For the comfort of such persons, Mr. Pattinson "supplied a criticism," one of the points of which was that the fundamental reaction of the chlorine process in question must be performed, either in an open furnace, in which case the chlorine yielded by it would be mixed with products of combustion, or "in a retort or close furnace," costly alike for fuel and for wear and tear. I replied that the apparatus in which that reaction is performed at Salindres is of the nature neither of an open furnace nor of a "close" one; and I have been surprised to find that there are those who have been unable to understand how this could be.

Our French friends have a proverb which declares that "a door must be either open or shut;" but it is not the case that a furnace must be either an "open" furnace or what we call a "close" one: meaning by an "open furnace," one the charge in which is heated by direct contact with the products of the combustion of the fuel employed, and by a "close furnace" one in which the charge is heated, without coming into contact with products of combustion, by heat applied to the exterior of the furnace, and transmitted to the charge contained therein through the material of which the furnace is constructed. There is of course another kind of furnace than either of these. A furnace which was in use before the dawn of history, the principle of which was known to and applied by pre-historic man, a furnace which exists to-day in greater numbers than any other, and which has rendered and is still rendering more important service to humanity than all other kinds of furnaces put together, is neither an "open furnace" nor a "close" one. Need I say that I refer to the baker's oven?

To prepare the staff of life, one has to roast small masses of a slightly fermented intimate mixture of flour and water. When he has charged these into his furnace, or "oven," the baker does not pour in upon them flame and heated products of combustion; and hence his furnace is not an open furnace. Nor does he apply heat to the exterior of his oven, and so roast his loaves by heat transmitted to them through the substance of its floor, walls, or roof; and hence his oven is not of the nature of a "retort or close furnace." What he does is to heat up his oven before his loaves are put into it, and the loaves are then roasted, or "baked," by means of heat which was applied to and stored up in the bricks of which the oven is constructed before the loaves entered it. The new chlorine furnace in question is upon precisely the same principle; and M. Pechiney and I, in speaking of it together, always call it the "four de boulanger."

It consists of a series of vertical chambers, the thickness of the dividing walls of which is considerably greater than the width of the spaces between them. Before charging these chambers with oxychloride of magnesium, products of combustion are passed through them, until their dividing walls are raised to a certain temperature. The products of combustion which have traversed the series of chambers go first through a regenerator and then to apparatus in which most of the heat still retained by them is utilised for the evaporation of the solution of magnesium chloride with which the process commences. When the dividing walls of the furnace have been heated sufficiently, the admission of products of combustion is arrested, and the chambers are charged, through doors at the top of each of them, with magnesium oxychloride in small lumps. By absorption of some of the heat previously stored up in the massive dividing walls of the furnace, these lumps of oxychloride rapidly become raised to such temperature that the oxygen of the air which is now admitted into the furnace can react upon them, with the result that their chlorine becomes replaced by some of that oxygen, and that there passes off a mixture of free chlorine, nitrogen, excess of air, and vapour of hydrochloric acid: which mixture passes first to apparatus in which its vapour of HCl is condensed out from it, its gases then passing on into mechanical bleaching powder chambers. When this replacement of the chlorine of the magnesium oxychloride by atmospheric oxygen has become complete or nearly so, the residual magnesia is discharged by doors at the bottom of each compartment of the furnace, and the furnace is then ready to be heated up again, for another operation.

The furnace invented at Salindres for the oxychloride of magnesium chlorine process thus avoids equally both the alternative difficulties, one or other of which Mr. Pattinson declared that the process "must" necessarily encounter. The chlorine liberated in that furnace is not mixed with products of combustion, nor is the material treated in it heated by the costly method of transmitting to it, through the substance of the vessel containing it, heat applied to that vessel externally. To which allow me to add that the furnace is not costly of construction; that it is subject only to a moderate amount of wear and tear; that it is obviously economical of fuel; and that there seems every likelihood that the manufacture of chlorine from chloride of magnesium is by no means the only purpose to which it will prove capable of being applied with advantage.

To the chlorine process for which this furnace was invented, Mr. Pattinson further objected that the chlorine yielded by it is hot chlorine, requiring to be cooled before it can be applied to the manufacture of bleaching-powder: as though black ash did not require cooling before it can be lixiviated; as though it were an objection, alike to the Leblanc soda process and to the ammonia soda process, that the finished product of each respectively must be cooled before it can be packed into casks; as though it were a grave fault on the part of our method of manufacturing sulphuric acid that the gases from the pyrites burners must be cooled before they can enter the vitriol chambers; and as though it could matter in the least whether the chlorine had to be cooled or not, provided only that it be finally yielded, as chlorine which has been absorbed by calcium hydrate and has so become a constituent of bleaching-powder, more cheaply than chlorine manufactured in any other way. I may nevertheless point out that the operation by which the chlorine is cooled is not performed for the express purpose of cooling the chlorine, as Mr. Pattinson's "criticism" would imply that it is, but would equally have to be performed even if the chlorine were not hot at the stage at which that operation comes in: the case being one of a process yielding two distinct products, which two products, at an intermediate stage of the process, are mixed together, but neither of which is of any value until it is separated from the other, so that the process by which both are manufactured is not completed until the operation by which their separation is effected has been performed, and that to say of that process that the operation by which its two products are separated is an essential part of it is to criticise it only in the sense, if any, in which it would be a criticism of the Leblanc soda process to say that its intermediate product, black ash, has to be separated into sodium carbonate and calcium sulphide, or in which it would be a criticism of the ammonia soda process to say that it is necessary in that process to split up the hydrogen-sodium carbonate intermediately produced in it into normal sodium-carbonate, vapour of water, and free carbonic anhydride.

The two products are of course free chlorine and vapour of hydrochloric acid. Whereas, per 100 parts of the chlorine contained in hydrochloric acid treated by the old Weldon process, 30 parts are yielded as free chlorine and 70 parts as the chlorine of calcium chloride, per 100 parts of the chlorine of hydrochloric acid treated by the oxychloride of magnesium process, 50 parts are obtained as free chlorine, and the other 50 parts as HCl: the aqueous acid obtained by the condensation of which HCl may either be neutralised by magnesia and made to yield another 50 parts of free chlorine, or be applied to any other purpose which may be preferred. To whatever purpose, however, that HCl be afterwards applied, it must be separated from the free chlorine which is liberated concurrently with it, and the operation by which it is so separated yields the chlorine cool: no operation for the special purpose of cooling the chlorine being thus required.

Even, however, if the chlorine had to be cooled by an operation having no other object, what would that operation amount to? Supposing the mixture of chlorine and other gases to be not much more than half as rich in chlorine as it often has been, the volume, at the temperature of the atmosphere, of the quantity of it corresponding to a ton of bleaching-powder would be about 28,000 cubic feet, or scarcely more than one-sixth of the volume of the quantity of gases which has to be cooled, in the manufacture of sulphuric acid, supposing the gases from the burners to contain 7 per cent. by volume SO_2, from the temperature of the combustion of pyrites to that at which they enter the vitriol chambers, per ton of pyrites burnt. A single firm in this district, the Newcastle Chemical Works Company, burning 30,000 tons of pyrites per annum, cools per annum, from and to the temperatures I have mentioned—without Mr. Alfred Allhusen, I venture to say, considering that he is doing anything specially wonderful in so cooling a quantity of gases so enormous—about five thousand millions of cubic feet of burner gases: being a quantity little, if at all, inferior to the total quantity of gases which would have to be cooled in connection with the oxychloride of magnesium chlorine process, if there were manufactured by that process absolutely the whole of the chlorine at present produced by all the chlorine manufacturers in the world put together.

In conclusion, just four sentences, if you please, with respect to the dilution of chlorine by other gases, and the manufacture of bleaching-powder in mechanical chambers. One point of Mr. Pattinson's "criticism" was that the chlorine yielded by the process of which I have been speaking, and that yielded by a certain other process, being dilute, cannot be used in ordinary bleaching-powder chambers. So might it have been objected in the early part of this century to the proposal to use coal gas as an illuminating agent, that it could not be burned in the ancient candle-sticks. The candle-sticks have gone; and I express a most deliberate and carefully-weighed opinion in saying that the primitive bleaching-powder chambers would have to follow them, even if there were no question of any change in the methods by which chlorine is manufactured. Mr. Pattinson said in the *Chronicle* that "it is always desirable, if not essential, to make it" (bleaching-powder) "with concentrated pure gas;" but that ancient superstition is not only not supported by experience of the use of dilute chlorine in mechanical bleaching-powder chambers, but all such experience goes to show that, even when manufacturing chlorine by methods yielding "concentrated pure gas," the wise course would be to deliberately mix air with that gas, in order to become able to absorb it by a method much more rational than the primitive method at present used exclusively everywhere, I believe, except at Salindres: by a method, that is to say, which not only avoids all loss of chlorine, and all nuisance arising from such loss, and is very much more rapid than the ordinary method, but which also yields much more stable bleaching-powder, and bleaching-powder much more uniform in quality, than can be manufactured by the old method, but which new method absolutely requires that the chlorine treated by it should be mixed with an inert gas, which may constantly carry off the heat liberated by the combination of the chlorine with calcium hydrate. Experience of that mechanical bleaching-powder chamber which Mr. Pattinson said that it had been "necessary to invent" for the absorption of the dilute chlorine yielded by the new processes in question, but which was really invented and used—long before any experiments on those processes had been commenced at Salindres—for the absorption of the chlorine remaining in the atmosphere of ordinary bleaching-powder chambers, fed by strong chlorine from the old Weldon process, at the moment of taking down their doors, has proved conclusively that to say of a method of manufacturing chlorine that the chlorine yielded by it is diluted by other gases, provided only that those gases are inert gases, not capable of being absorbed by calcium hydrate, and that the degree of dilution is not too great, is to say that the chlorine manufactured by that process is better adapted, and not less well adapted, to the manufacture of good bleaching-powder, than if it were not diluted by other gases; and that to object to such a process that the chlorine yielded by it cannot be used in the old bleaching-powder chambers, is as though one should maintain that the modern railway system is inferior in advantage to the stage-coach system of our fathers, on the ground that railway locomotives cannot be run on ordinary roads.

DISCUSSION.

The Chairman: The formal discussion of Mr. Weldon's paper will take place, of course, at our next meeting; but as Mr. Weldon may not be able to attend our next meeting, perhaps those of the members present who would like to put any questions to him on the subject of his paper will do so now, while he is here to answer them.

Mr. John Pattinson: It would be interesting to know on what scale bleaching-powder is now being made at Salindres by the oxychloride of magnesium process. I presume that the magnesium chloride is obtained by decomposing by magnesia the ammonium chloride of the ammonia soda process?

Mr. Stuart: Can Mr. Weldon give us any idea of the form and principle of the mechanical bleaching-powder chamber of which he has spoken? The Chief Inspector under the Alkali Acts has been pressing manufacturers to adopt some method of dealing with the residual chlorine remaining in the present chambers when the charge in them is ready to be packed, and his proposals on this point have not met with very general approval. Perhaps M. Pechiney's mechanical chamber might be a solution of the difficulty. I should also like to ask which process of manufacturing chlorine at present offers the greatest promise of future success—the oxychloride of magnesium process, or the chloride of manganese process?

Mr. Morrison: What length of time is required for the operation of manufacturing bleaching-powder in mechanical chambers?

Mr. Nixon: What are the practical difficulties of decomposing oxychloride of magnesium by atmospheric oxygen? Is the whole of the oxygen of the air used absorbed? And is the bleaching-powder made from the dilute chlorine got up to full English strength?

Mr. Weldon: Up to the present time, the successive installations at Salindres for the oxychloride of magnesium process have been upon a semi-industrial scale; but a large industrial plant for it is now in course of construction there. The magnesium chloride hitherto used at Salindres as the raw material of the process in question has been that obtained in the salt gardens of MM. Pechiney et Cie., at Giraud, in Camargue, as the mother-liquor from the manufacture of salt, by natural evaporation, from sea water. The mechanical bleaching-powder chamber in use at Salindres was devised for the express purpose of absorbing the chlorine contained in ordinary bleaching-powder chambers at the moment of taking down their doors. It is upon the principle of the horizontal revolving cylinder which was tried at Gerard's Bridge, St. Helens, many years ago. Mechanical bleaching-powder chambers, however, cannot give good results with strong chlorine. When the chlorine used in mechanical chambers is sufficiently diluted by inert gases to carry off the heat generated by the reaction of the chlorine on calcium hydrate, mechanical bleaching-powder chambers yield as strong bleaching-powder as can be obtained in any other chambers. The time required for the operation of manufacturing bleaching-powder in mechanical chambers depends chiefly on the rate at which chlorine can be sent into the chambers, without raising the partially formed bleaching-powder to too high a temperature; and this depends chiefly on the degree of dilution of the chlorine. The one of the two processes referred to by Mr. Stuart which M. Pechiney and I believe to be the most promising, is the oxychloride of magnesium process. It presented a good many practical difficulties at first; but, thanks to M. Pechiney, and to the engineer of Salindres, M. Boulouvard (the inventor of a very successful form of the ammonia soda process), I believe they may all be regarded as having now been overcome. The whole of the oxygen of the air employed can be made to react on the magnesium oxychloride, but it is better to work so that only about half of it shall react.

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Meeting held March 4, 1885.

MR. CHARLES HUNT IN THE CHAIR.

NEW AND LITTLE-KNOWN APPLICATIONS OF THE NITROMETER.

BY ALFRED H. ALLEN.

Doubtless many of the members of the Society of Chemical Industry are in the habit of using the nitrometer, and if they have employed it for one particular purpose have probably been struck with its applicability to certain others which fall within the range of their requirements. Hence, I am prepared to find that most of the applications of the nitrometer which I intend to refer to this evening have already been made by individual members of the Society, and my excuse for bringing the matter forward must be the desirability of causing these applications made by individual members to come into more general use. Still, I believe I may claim that certain uses of the nitrometer to which I desire to call attention are strictly novel and original.

As long ago as 1847 (*Phil. Mag.* [3] xxx. 426), the late Walter Crum described a method of estimating the *nitrates* and analysing *gun cotton*, which consisted in passing up the nitrated substance, together with some water and sulphuric acid, into a graduated tube filled with mercury. The nitric oxide liberated in the reaction was subsequently absorbed by a strong solution of ferrous sulphate. The method of manipulation was greatly improved by Frankland and Armstrong in 1868 (*Journ. Chem. Soc.* xxi. 101), and recommended by them for the estimation of *nitrates in water*. Their use of a tube open at one end and closed at the other by a tap surmounted by a cup greatly facilitated the introduction of the nitrate and acid. This apparatus, which *may* be called the Frankland-Armstrong tube, would have come into more general use had the agitation of the lighter liquids with the mercury been more easy to effect, but the accuracy of the results yielded by it have been fully proved by Mr. R. Warington (*Journ. Chem. Soc.*, xxxv. 376), who also found that the presence of chlorides did not interfere to any notable extent with the amount of gas obtained.

Mr. G. E. Davis states that the idea of applying the mercury process to the assay of *nitrous vitriol* was suggested to him by Dr. John Watts in 1872; and, in a paper read before the Faraday Club on January 22nd, 1878, Mr. Davis fully described the use of the Frankland tube for the assay of vitriol, which tube he used in connection with a mercurial trough. In February, 1878, Mr. Davis informs me that he connected Frankland's tube to a piece of combustion tube by means of india-rubber tubing, and used the two side by side in a burette-stand. This arrangement constituted the first form of the instrument we now call the nitrometer.

The name "nitrometer" is due to Dr. Lunge, who in 1878 published and illustrated a description of his instrument (*Berlin Berichte*, xi. 436; *Dingler's Polyt. Journ.*, 228, page 447; *Chem. News*, xxxviii. 19).

To Dr. Lunge, then, belongs the credit of the first published description of the nitrometer, but the same instrument appears to have been devised independently at a somewhat later date by Mr. Campbell, then of the Hebburn Chemical Works.

Lunge's nitrometer possessed one decided improvement in the use of a three-way tap, which arrangement appears to have been adopted from Bunte's gas apparatus. When the nitrometer is employed for the estimation of nitrates, or the assay of nitrous vitriol, the three-way tap greatly facilitates the removal of the acid liquid at the end of an experiment, without necessitating the complete emptying of the nitrometer. Later on I propose to show that the nitrometer furnished with a three-way tap has such important uses and advantages of its own, that it almost deserves to rank as a distinct piece of apparatus.

As ordinarily employed, the nitrometer-stand is so arranged that the open or reservoir-tube must be removed from its position and brought into close proximity to the graduated tube, before the measure of gas in the latter can be read off, and it is therefore an advantage to have the stand so constructed that the closed and open tubes of the apparatus, which I propose hereafter to refer to as the "nitrometer" and "reservoir" tubes respectively, should be in close proximity.

Another inconvenience attending the use of the nitrometer is the persistent mercurial froth which remains after the completion of the reaction, and prevents the reading of the level of the mercury in the nitrometer tube from being taken without delay. When great accuracy is not required, this may be avoided by employing a tolerably wide tube; but, for ordinary purposes, it is preferable to adopt a plan suggested by me in a paper on the estimation of nitrates by the nitrometer, published in 1880 (*Analyst*, v. p. 181). In that paper, I proposed to make a mixture of sulphuric acid and water, in the same quantity and proportions as the acid and solution of nitrate are employed in the nitrometer-tube, and to pour this mixture on the top of the mercury in the reservoir-tube. It is then possible at once to bring the aqueous liquids in both tubes to the same level, and the incomplete separation of the mercury from the aqueous liquid becomes a matter of indifference. In the same paper, I proposed to make an experiment with a second nitrometer, in which a definite *amount of nitre* was used. *By noting* the volume of nitric oxide evolved in this apparatus, and comparing it with that obtained in the case of the liquid of unknown composition, it becomes unnecessary to make any correction for pressure or temperature; and the standard quantity of gas, when once obtained, can be kept in the tube without alteration for any length of time. If this course be pursued, it is desirable to keep a little strong sulphuric acid in the cup, to prevent any possibility of leakage.

Before leaving the question of the estimation of nitrates in the nitrometer, I may point out a source of error which appears not unfrequently to have been ignored. This is, that strong sulphuric acid has a notable solvent power for nitric oxide, and hence the full amount of gas cannot be obtained unless the sulphuric acid be diluted below that point at which its solvent power for nitric oxide becomes appreciable. Warington and Thorpe both recommend that the strong sulphuric acid and aqueous liquid should bear the proportion of three measures to two, and my own experience is in favour of this condition.

In experiments on nitrous vitriol, made by Mr. T. Bayley at my request, to test the extent of the error from absorption of nitric oxide, he found that in the case of acid of a density of about 1·62 the extra gas obtained after further diluting the acid was fully 0·5 cc. This source of error becomes more serious when stronger acid is employed, and is not improbably the cause of the *low results obtained* when an excessive quantity of acid is used for the assay of dynamite in the nitrometer.

For the examination of *dynamite* and *gun-cotton*, Lunge places about ·35 gramme of the sample in the cup of the nitrometer, and dissolves it in 5cc. of concentrated sulphuric acid. The liquid is then allowed to enter through the tap, and is agitated with the mercury in the usual way. Probably better results would be obtained if a few centimeters of water were added after the completion of the reaction.

Hitherto all employers of the nitrometer appear to have regarded mercury as the only liquid with which the instrument could be filled, and of course this is actually the case in the estimation of nitrates, since the mercury itself takes part in the reaction.

For many purposes, however, the mercury can be conveniently replaced by a dense aqueous liquid, saturated brine being most generally suitable, and the following are some of the applications to which I have put the nitrometer when filled with such a liquid.

The assay of *spirit of nitrous ether* and kindred preparations for the proportion of ethyl nitrite is a problem which has engaged the active attention of pharmaceutical chemists. Hitherto the processes for the purpose have not been very satisfactory, the one giving results nearest to the truth being that of Eykman, which consists in treating the sample with a solution of ferrous sulphate and dilute sulphuric acid, heating the mixture, and collecting the nitric oxide gas evolved. Though the process is theoretically perfect, there are certain sources of error in practice, and the apparatus is somewhat complicated, and requires skill in management. Mr. D. B. Dott has proposed to estimate ethyl nitrite by treating it with potassium iodide and dilute sulphuric acid, and estimating the amount of iodine set free by titration with a standard solution of sodium thiosulphate. The reaction which occurs in this process is expressed by the equation :—

$$(C_2H_5)NO_2 + KI + H_2SO_4 = KHSO_4 + (C_2H_5)OH + I + NO.$$

In making experiments on Dott's method it occurred to me to measure the nitric oxide gas evolved, instead of titrating the iodine liberated, and as the reaction occurs completely and promptly at the ordinary temperature, the nitrometer is an apparatus well suited for the purpose. I have now had a large experience in the process, which is best performed in the following manner :—A nitrometer should be filled with strong brine, and 5cc. of the sample to be tested should then be placed in the cup of the nitrometer, and allowed to enter through the tap, taking care that no air gets in at the same time. About 5cc. measure of a strong solution of potassium iodide is next allowed to enter, and this is followed by about 5cc. of dilute sulphuric acid. Effervescence immediately ensues, and, if the tube be vigorously agitated at intervals, the reaction is complete in a few minutes, when the level of the liquid in the two limbs of the nitrometer is adjusted, and the volume of nitric oxide gas read off. There is no difficulty in confining the reaction to the liquid in the upper part of the closed limb of the nitrometer. The nitrometer method has been proved to give very good results with pure *sodium nitrite*, prepared from silver nitrite, and employed in known amount, and may be advantageously used for the assay of commercial nitrite of sodium. With some specimens of sweet spirit of nitre a considerable volume of nitric oxide is produced, and iodine liberated before adding the acid, the reaction probably depending on the presence of free acid in the sample. The results obtained in the nitrometer are remarkably constant, and the method furnishes a very easy means of assaying sweet spirit of nitre with considerable accuracy, which is further increased if a correction of 1·5cc. of NO be made for solubility of the gas. The process does not occupy ten minutes, including the calculation, and actually measures the nitrous compounds present in the sample, instead of leaving their proportions to be inferred from a more or less complex reaction, such as the reduction of permanganate, etc. When strictly accurate results are not required, as is rarely the case in assaying such a preparation, the volume of gas need not be corrected for variations of pressure, temperature and tension of aqueous vapour, etc. 1cc. of NO at the *ordinary* conditions of pressure and temperature corresponds to ·003184 gramme of ethyl nitrite,[*] and hence 5cc. of samples of spirit of nitrous ether of reasonably good quality evolve from 25cc. to 40cc. of nitric oxide.[*]

Another useful application of the nitrometer is for the estimation of *urea* in its urine. For this purpose the tube is filled with brine as before, and 2cc. of the sample introduced through the tap. This is followed by 10cc. of the usual solution of bromine in caustic soda.[†] Before the bromised soda has time to sink through the brine it comes in contact with the urine, and the great bulk of the nitrogen gas is liberated. By now placing a clip on the india-rubber connecting-tube and agitating the contents of the nitrometer tube vigorously, the reaction is completed, and the volume of gas may be read off. Not unfrequently the reading is delayed by the existence of froth on the surface of the liquid in the closed tube, but this may be instantaneously got rid of by introducing a few drops of alcohol through the tap. In some respects it is preferable to bring the bromised soda and urine in contact with each other in a small bottle or flask fitted with a perforated cork, and connected by narrow india-rubber tubing with the nose of the three-way tap of a nitrometer filled with brine or water (Fig. 2). A nitrometer is much better for this purpose than the device of a burette or graduated tube immersed in a tall jar of water, as is employed by Dr. Squibb and others.

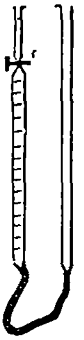

FIG. 1.

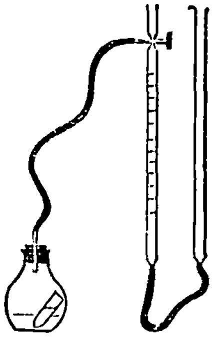

FIG. 2.

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[*] I have had a special nitrometer (Fig. 1) constructed for me for the assay of nitrous ether preparations. The graduated tube holds 50cc., and the cup is narrow and cylindrical, and has marks at 5cc. and 10cc. above the tap. This arrangement renders the use of a pipette or other separate measuring arrangement unnecessary, as the cup can be filled to the 5cc. mark with the sample, and thus a definite amount taken for the analysis. In this case a three-way tap is of no advantage. Messrs. Cubley and Preston, of High Street, Sheffield, are now prepared to supply these nitrometers to those desirous of employing the process mentioned in the text. With regard to the titration process of Mr. Dott, if the treatment with potassium iodide be effected in an open basin in presence of air, as is recommended by him, the amount of nitrite found is liable to be very seriously in error; but if air be excluded, Dott's method gives fair approximate results, somewhat in excess of the truth. The process can be advantageously employed on the solution which has already been decomposed with potassium iodide in the nitrometer. The nitric oxide is allowed to escape into the air, and the brown liquid is washed into a basin, and at once titrated with decinormal thiosulphate.

[†] 100 grammes of caustic soda are dissolved in 250cc. of water and 25cc. of bromine cautiously added to the well-cooled liquid. In some respects it is preferable to dilute the 10cc. of this solution with an equal measure of water before introducing it into the nitrometer.

The last arrangement is well suited for the estimation of *carbonates*, the sample being placed in the flask, and the acid for its decomposition contained in a small tube in the usual way. This amounts to a suggestion to employ a nitrometer in place of the measuring tubes of Scheibler's carbonic acid apparatus. Of course the usual corrections for solubility of the gas in the acid liquid, etc., apply as much in one case as in the other.

The nitrometer may also be very conveniently used for the assay of commercial *peroxide of hydrogen*, for which it was first employed by Mr. C. E. Davis, who filled the nitrometer with mercury and operated on 1cc. of the sample, which he agitated with 5cc. of a saturated solution of potassium bichromate. Under these circumstances Mr. Davis found the peroxide of hydrogen to be split up into water and oxygen, the bichromate remaining unchanged, and this observation has been subsequently confirmed by Mr. W. H. Symons. It is usual to express the strength of peroxide of hydrogen in terms of volumes of oxygen obtainable therefrom, and hence the nitrometer is well suited for its assay.

According to my own experience, the reaction of bichromate of potassium on peroxide of hydrogen does not occur so promptly or completely as could be wished, and I have therefore substituted for it an acidulated solution of permanganate of potassium. I prefer to operate as follows:—

The nitrometer is filled with a saturated solution of sodium or magnesium sulphate.* 1cc. of the sample is then introduced through the tap, and this is followed by a strong solution of potassium permanganate, which has been previously acidulated with sulphuric acid. The reaction occurs very promptly, and the permanent coloration of the contents of the tube suffices to show that an excess of the permanganate has been employed. When acidulated permanganate is used the reaction is as follows:—

$$2KMnO_4 + 5H_2O_2 + 3H_2SO_4 = K_2SO_4 + 2MnSO_4 + 8H_2O + 5O_2.$$

Mr. Kingzett and these chemists have independently proved the truth of the equation by adding standard permanganate to hydrogen peroxide (in presence of free acid) contained in a Woulffe's bottle arranged so that the evolved oxygen could be collected and measured. The accuracy of the results obtained by permanganate was checked by estimating the iodine liberated by the hydroxyl from potassium iodide. Hence, when acidulated permanganate is employed for decomposing hydroxyl, only one-half of the oxygen gas liberated comes from the samples, the other half being derived from the re-agent. Therefore, 1cc. of a sample of peroxide of hydrogen, described as of "10 volumes strength," ought to give 20cc. of oxygen in the nitrometer. If an acidulated solution of potassium bichromate be substituted for the permanganate the reaction occurs very promptly, a double volume of oxygen being evolved, but for some unexplained reason the results are sensibly lower than those obtained with permanganate.

While the nitrometer is frequently used as an instrument in which to evolve and measure gas, it has not met with such a wide application as it deserves for the purpose of *absorbing* gases. But, as a matter of fact, by the aid of two of Lunge's nitrometers, having their three-way taps joined by a short piece of india-rubber tubing, we can extemporise an apparatus for gas analysis capable of a variety of applications. This is practically done by Bunte in his gas apparatus, but, curiously enough, in the method of working he prescribes, he makes little or no use of his cup, preferring to introduce his re-agents by a somewhat complicated manipulation through a tap at the bottom of the nitrometer. In Elliott's gas apparatus full use is made of the cup arrangement, but instead of employing a three-way tap, Elliott uses a special arrangement for transferring the gas to the measuring tube.

Having not unfrequently to make analyses of blast-furnace and Siemens gases for technical purposes, the importance of having a rapid and moderately accurate method of gas analysis has forced itself on my attention, with the result that I have devised a very simple apparatus, combining the various advantages of the instruments of Stead, Bunte and Elliott. The following is a description of the apparatus in its simplest form.

The measuring tube A (Fig. 3) ends at the top in a capillary tube bent at right angles, and furnished with a tap E. Two platinum wires are sealed into the upper part for passing an electric spark when required. The lower end of the tube is drawn out and attached by india-rubber to a T-piece, by which it is connected to the parallel open tube B and to the reservoir D. In cases where the apparatus is to be used with mercury, the vessel D should be replaced by a reservoir sliding in vertical grooves, and adjustible by means of a pulley. The tubes A and B are enclosed in a glass cylinder, which is filled with water, the temperature of which is usually found in practice to remain constant during the whole time required for a gas analysis. The capillary tube at the top of A is connected by a few inches of narrow india-rubber tubing with the three-way tap of a nitrometer. The nitrometer-tube I prefer to employ has a globular enlargement just below the tap, and is furnished at the bottom with two taps L and M, by the latter of which it is connected with a reservoir of water N. A Lunge's nitrometer can be used as a makeshift for the special tube shown in the diagram, clips being substituted for the taps at the bottom. The apparatus can be worked equally well with either mercury or water, but to avoid repetition, I will describe the manipulation when water is used, and assume the gas to be analysed to be a mixture CO_2, CO, CH_4, C_2H_4, O, H, and N.

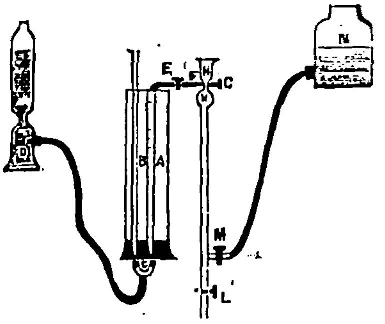

FIG. 3.

Before commencing an operation the nitrometer is detached from the measuring tube, the tube E is opened and the reservoir D raised, so as to drive out the whole of the air from the measuring tube, and its capillary continuation E is then closed, the reservoir lowered, attachment made with a receptacle containing the gas to be analysed, when, on the tap E being re-opened, the gas flows into the measuring tube. E is closed and the height of the reservoir D adjusted until the water in A and B stands at the same level,

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* If brine be used there is a tendency to error from the evolution of chlorine.

The measure of the gas taken for an experiment is then read off, and the temperature of the surrounding water and height of the barometer observed. As the gas only comes in contact with the water for a short time, and is not agitated with it, only a very trifling loss of CO_2 by absorption will occur, and this may be further diminished by employing strong brine instead of water. The nitrometer is now filled with water, some water being also placed in the cup, and the three-way-tap is opened so as to allow the india-rubber tube F to become filled with water, and while this is dropping connection is made with the capillary tube of the measuring vessel. The three-way-tap is then turned so as to open a communication between A and K, the tap M closed and L opened, when the gas will flow from A into K. A few drops of water from A are allowed to follow the gas. E is then closed, some of a concentrated solution of caustic potash poured into the cup, and the three-way-tap opened so as to allow the liquid to flow into the nitrometer, when L is closed. The globular enlargement at K, an arrangement for which I am indebted to Dr. A. H. Elliott, causes the solution to spread as a thin film over the surface of the glass, and insures rapid and perfect absorption of the CO_2. In this way the minimum quantity of the reagent can be employed. Practically, absorption is known to be complete by no more reagent being sucked in from the cup, and my experience shows me that the reaction is almost instantaneous. If, however, any doubt exist on this point, all the taps should be closed, and the nitrometer detached and shaken up. The tap M is then re-opened, connection again made between K and A, and the residual gas driven back to the measuring vessel, where its volume is observed, the decrease of course being due to CO_2 absorbed. The whole manipulation is extremely simple, requiring only a few minutes up to this point. For the absorption of the other gases the same series of manipulations is required, but before proceeding to the next step it is necessary to remove the potash solution from the nitrometer. E being closed this is done by turning the three-way tap so as to establish communication between H and K. M is closed and L opened, and water poured into the cup until the nitrometer is thoroughly flushed out. Then on turning the three-way tap so as to make communication between K and A, and opening E, the gas is again drawn into the nitrometer and is ready for further treatment in the following order:—

a. The CO_2 having already been absorbed by a strong solution of potash;

b. A saturated solution of bromine in bromide of potassium is introduced to absorb the hydrocarbons of the ethylene series.

c. A little potash is allowed to enter to absorb any bromine vapour, and the free oxygen is then absorbed by an alkaline solution of pyrogallic acid.

d. The nitrometer having been flushed, the carbonic oxide is absorbed by a strong solution of cuprous chloride in hydrochloric acid. The absorption occurs with great facility, and on then running in water through the tap any hydrochloric acid vapour is readily absorbed at the same time that the cuprous solution is washed out.

This completes the possibility of analysis by absorption, and hence the next step is to add oxygen to the residual gas, pass an electric spark, and note the contraction and amount of CO_2 formed in the usual way, which observations give data for calculating the proportions of hydrogen, marsh-gas, and nitrogen. The measuring tube, parallel tube, and reservoir are practically identical with Stead's arrangement. The method of introducing and using the re-agents and the employment of the globular enlargement are due to Elliott.

Of course strictly accurate results are not obtainable if water be employed in the measuring tube, but the error due to this cause may be reduced so as in many cases to be practically insignificant if brine be employed instead of water in this part of the apparatus.*

It is possible to conceive a still simpler form of apparatus by which useful rough analysis of gases might be made. This would be to dispense altogether with the separate measuring tube, and have the nitrometer graduated and furnished with platinum wires.

Perhaps there is no chemical operation more difficult to describe than that of the manipulation with an instrument for the analysis of gases, but in practice the mode of using the apparatus in question will be found exceedingly simple, and to anyone in the habit of working with gases means will readily suggest themselves for increasing the accuracy of the results. These precautions I have purposely omitted, as I have nothing new to say upon them, and my object in bringing the apparatus before the Society of Chemical Industry is to call the attention of its members to a ready means of making technical analyses of gases with a reasonable approach to accuracy, and not to attempt to replace what I may call the standard methods of gas manipulation familiar to all scientific chemists.

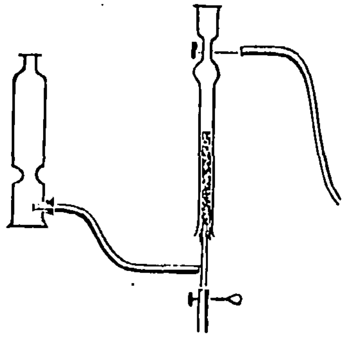

Fig. 4.

In conclusion, I may call your attention to some qualitative applications of the nitrometer with a three-way tap, which instrument furnishes an extremely ready means of obtaining certain gases in a state of purity. For instance, if the lower end of a Lunge's nitrometer tube be cut off and the opening closed with a perforated cork fitted with a T-piece, one branch of which is closed by a clip and the other communicates with the reservoir, we have a convenient apparatus for generating at will small quantities of such gases as can be readily evolved without application of heat (Fig. 4). Thus, for example, if some granulated zinc be placed in the nitrometer and the instrument filled with water, on allowing acid to enter through the cup hydrogen perfectly free from air will be evolved, and can be utilised in any manner desired by opening the three-way tap, to which a piece of india-rubber tubing has previously been attached. The T-piece at the bottom enables the dense solution of zinc to be tapped off when desired, and so the apparatus can always be kept full of hydrogen and in order for use. For small quantities of sulphuretted hydrogen the apparatus is also well adapted, and by means of the T-piece any choking up by ferrous sulphate can be wholly avoided. Carbon-dioxide also can readily be generated in the nitrometer tube, and if a small quantity of oxygen be required, as, for instance, in a gas-analysis, it can be obtained instantaneously and in a state of purity by the reaction between acidulated permanganate and peroxide of hydrogen.

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* I find that 100cc. of strong brine at the ordinary temperature dissolve 29·0 cc. of CO_2, whereas pure water would dissolve about 100cc. Messrs. Cubley & Preston are prepared to supply the apparatus for gas analysis described in the text.

It is the three-way tap which renders the nitrometer suitable for a large number of the purposes suggested.

In conducting the numerous experiments of which this paper is the outcome, I have been greatly aided by Mr. J. H. Worrall, to whom my best thanks are due.

DISCUSSION.

Professor **Tilden**: I think Mr. Allen is to be congratulated on the useful test he has introduced for determining the amount of nitrite in "sweet spirit of nitre," and I think pharmacists will be glad to avail themselves of it. In some earlier experiments of mine, I endeavoured to make use of the well-known decomposition of urea by nitrous acid, which results in the evolution of the whole of the nitrogen of the nitrite together with the nitrogen and carbonic acid from the urea. The relatively large volume of gas thus obtained makes the estimation of small quantities of nitrite possible, and increases the accuracy of the determinations. I venture to suggest to Mr. Allen that he should try this re-action. I feel tempted to criticise the process of gas analysis we have just seen illustrated, but I presume it is not expected to yield accurate results, and would be used only for technical and comparatively rough purposes. Its most serious defect appears to be the very large bulk of the liquid re-agents which is used.

Mr. **Staveley**: I should like to ask Mr. Allen if he has had any experience in the determination of the benzene present in illuminating gas. This has now become a very important point in the gas industry, and especially in the industries which have recently been started for the obtaining of benzene as the principal product in carbonising coal. There have only been two or three analyses of coal gas published in which benzene has been given. One was by Bunsen, and he gave about 1·33, and the other by Berthollet, of Paris gas, in which he gave a very large quantity, amounting to 3½ or 4 per cent. of benzene. It is very desirable that we should have an exact method of determining this. I believe the method Bunsen followed was to absorb the paraffins by bromine water, and the benzene by fuming sulphuric acid.

REPLY.

In reply, Mr **Allen** said: With regard to the estimation of urea, the production of the apparatus just shown was an illustration of the remark with which I commenced my paper—namely, that various *modifications of the nitrometer had doubtless suggested* themselves to individual chemists. I have tried the re-action suggested by Dr. Tilden, for the assay of nitrites by treatment with urea, but certainly did not obtain encouraging results, the re-action being too slow. This, however, may have been due to my using an insufficient quantity of acid. But the fact that the gas evolved would be a mixture of nitrogen and carbonic acid, would, in my opinion, be a disadvantage, as it would always necessitate operating over mercury, and hence would do away with the advantages obtained by substituting brine. Respecting the remarks of Dr. Tilden, as to the loss of soluble gases in working the gas apparatus, I may point out that agitation, under the conditions described, enables one to use the minimum quantity of each re-agent requisite for complete absorption. Literally, to effect the same set of absorptions with Stead's *arrangement*, the gas would have to be brought in contact with not less than 300 or 400 centimetres of liquid. Unless, therefore, we revert to Bunsen's method of absorption by solid re-agents, the mode of manipulation described by me seems the most free from objection. With regard to the question about the estimation of benzene in coal-gas, I am sorry I am unable to give a definite reply, but would suggest that some method, based on the absorption of benzene and its homologues by fuming nitric acid, would probably be found to answer

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A meeting of this Section was held in the Rooms, 207, Bath Street, on Tuesday, 3rd March, 1885.

PROFESSOR FERGUSON IN THE CHAIR.

ON THE PROCESSES EMPLOYED IN ITALY FOR THE EXTRACTION OF OILS, ETC., FROM BITUMINOUS ROCKS IN THAT COUNTRY.

BY MR. JAMES MURRIE.

Italian Bituminous Deposits.—The Italians, during the past generation, have, as you are no doubt aware, been making strenuous exertions to develop their internal resources and to occupy a prominent position in the commercial world. So far they have made a most rapid advance in manufacturing for their own requirements, and the public and commercial undertakings initiated and carried out during this period will compare favourably with the advancement made by any other civilised nation.

These results are mainly due to the enlightened and far-seeing legislation of their Government, which, while aiming at making Italy one of the most tolerant and free countries in the world, have held out every inducement and facility to foreigners to embark in their undertakings, or to initiate and develop industries under the surveillance and direction of their own employés, irrespective of nationality. Hence a very great proportion of the works carried on have been begun and conducted by imported skill and capital; and it may be added that it has been the heavy taxation on this same capital which has been mainly instrumental in placing Italy in its present sound financial position.

Unfortunately for the continued prosperity of the country, it lacks one of the most important agents by which its almost illimitable mineral wealth can be made the most of—namely, a coal-bed. Although possessed of a considerable number of carbonaceous deposits, mainly in the form of lignites, these are either too poor in heating properties, or too limited in quantity to compete with imported sea coal, the cost of which is a heavy tax on the finished products of manufacture. The Italian Government, fully alive to the importance of having reliable data on the various mining operations carried on, have established a corps of engineers, having headquarters in the principal centres, whose duty it is to examine and report on any new bed of mineral, and to give an abstract of the processes in operation. The result is embodied in an annual report, which is published in book form and contains a fairly accurate synopsis of the work done and in progress. To those interested in this department of Italian industry the report in question forms an excellent reference.

Apart from this, there are a considerable number of foreign adventurers or explorers in the country, who are keenly alive to the value of any discovery of any new field of mineral capable of being turned to account commercially, and who, in a majority of cases, are the promoters or indirect means of floating new concerns, in connection with which it cannot be too well known that the country has been explored throughout by Government experts, and, besides, is peopled by a class who know the value of commercial commodities as well as the passing or temporary resident. Any reported discovery cannot therefore be too well scrutinised, even although backed up by scientific reports.

Oil and bituminous indications do occur over a very wide area at different points throughout the country, which have received more or less attention from those interested, and which has given rise to the popular belief that a belt of oil passes through the Appennines in the direction of Roumania, rising to the surface near Bucharest. This theory has received the support of several British geological experts, which, if true, and capable of being extracted on the American system, would undoubtedly prove a great stimulus to Italian industry, and be the means of making Italy the leading nation in the Mediterranean.

The oil and bituminous indications which have received the greatest share of attention are those found in a spur of the Appennines, known as the Abruzzo, in the province of Chieti, and about twenty miles inland from the town of Pescara, on the Adriatic. The locality can be determined by a reference to a map; an imaginary line, drawn between Mount Etna in Sicily and Mount Vesuvius at Naples, and continued towards the Adriatic, will pass through the district in question, the exact locality being about 90 kilometres beyond Naples.

The oil indications occur in the form of a flux, or tarry, sandy mass, which exudes from the mountain side after heavy rains, and the bituminous indications in the form of asphaltic rock. The asphaltic deposit I purpose describing more particularly, occurs in the slope of the mountain, dipping into the southern side of the river Pescara, in the vicinity of the town of San Valentino. The minerals of the district consist of broken limestone, sandstone, marl, and clay, gypsum and limestone with indications of sulphur, and the geological formation appertains to the eocene age. The bituminous rocks occur in two distinct forms—namely, a strictly asphaltic rock, consisting of limestone and earthy matter impregnated with bitumen, the bitumen acting as a cement in holding the body together; a bituminous stone resembling the American prairie oil-stone in appearance, the rock in this case acting as a carrier of the bitumen.

The asphaltic rock is formed on the lower slope of the mountain, rarely at a greater altitude than 2000ft., and the oil-stone, or bituminous stone, invariably at a greater altitude. The asphaltic rock is strictly a superficial deposit, which occurs very irregularly, sometimes in isolated boulders of limestone, but more frequently in surface soil or thin sheets of nearly pure bitumen between the earthy soil and a lower bed of clay. In the case of limestone, the rock usually resembles a foliated clay, or rottenstone, cemented together by the bitumen, and in a few instances, nearly pure limestone uniformly saturated with the bitumen, the proportion rarely exceeding eleven per cent., and the average about five per cent.

The bituminous stone found at a higher altitude is more uniform in its nature, consisting of calcium and quartz, closely associated with each other, filled with myriads of microscopic cavities, which contain the bitumen. This deposit occurs in veins running parallel with one another in line with the slope of the mountain; the breadth of the vein is very variable, in some cases 10ft., and in others—not one mile apart—100ft. The depth averages 60ft. The proportion of bitumen is more uniform than in the lower deposit, the average being three and a half per cent.

Manufacture of Products from Asphaltic Rock.—The asphaltic rock is, from its peculiar formation, specially adapted for asphaltic purposes; as such it has been fairly successful, and through the instrumentality of an Ancona merchant, W. von Bremen, has found its way to nearly every part of the globe. The process employed in its preparation corresponds to the kindred operation carried on in the neighbourhood in the preparation of mastic for pavements. The Italian process is essentially as follows:—The rock on being quarried or excavated is conveyed to the factory by means of donkeys, mules, and women, thence is broken up into pieces suitable for passing through the disintegrator or pulveriser, converting the stone into a powder the consistency of finely ground sand, in which condition it flows into a revolving cauldron, maintained at a constant temperature of 400° F., and is there mixed with a proportion of bitumen, the percentage depending on the quality of the rock, and the purpose to which it is to be adapted. For flooring and roofing purposes about 6% is usually added.

After the mixture has been thoroughly agitated, the semi-viscid mass is run into moulds, not unlike a cheese box; on cooling it hardens sufficiently to admit of handling in railway transit, when on reaching its destination and being prepared for use, it is again broken up, re-melted in small boilers, and spread over the flooring or pavement bed, trowel fashion.

It forms a capital roofing material for a warm country; being a good non-conductor, it keeps the chambers cool and refreshing in summer, and effectually excludes moisture from entering in winter. For flooring, again, it prevents damp, and being hard enough at the ordinary temperature to resist abrasion, answers well for many purposes where a clean and dry floor is an important consideration. Its employment as a pavement and cement for reservoirs and canals is too well known to need repetition. In manufacturing paving bricks, the semi-liquid powder run from the revolving cauldron is conveyed to a hydraulic press, fitted with a series of recesses and dies to stamp the brick into the desired shape; the recesses are made to contain five or six times the capacity of the finished brick, a pressure of about two tons per square inch, applied gradually, being sufficient to give the brick the necessary degree of hardness. A pavement formed of these blocks, made of a neat pattern, say having hexagonal faces, about 6in. in diameter, and about 2½in. thick, presents an exceedingly attractive and artistic appearance, and at the same time excels any other in durability and cleanness. The high cost of manufacture and heavy transport charge has prevented the introduction of these bricks on any large scale, although often met with in the courtyards of the more important Continental cities, particularly Venice, Rome, Florence and Paris. The moist atmosphere of London has prevented its introduction to any extent in that city. It may be added that the odour given off in melting and laying bituminous mastic is rather agreeable than otherwise, and forms a striking contrast to the coal-tar asphalte paving operations carried on in this country.

Extraction of Bitumen.—Closely associated with the preparation of the mastic and forming a companion to it, is the extraction of the bitumen from the rock, either for mixing subsequently with the asphaltic powder already described or for other purposes. The process at first sight seems to be a very simple one, the mineral only having to be heated in a suitable vessel to allow the bitumen to liquefy when heated, and descend or flow downwards through the mineral, escaping by a suitable outlet placed in the lowest part of the chamber. In reality, however, the operation is comparatively difficult, and has proved so to several companies who have erected plant with the expectations of extracting the bitumen on the same line as employed elsewhere. This becomes apparent when we consider that to heat the asphaltic rock means melting the cementing or adhesive body; the earthy matter or broken limestone having no longer any tendency to retain its rigidity crumbles up and subsides to the lower part of the vessel, forming a semi-viscid mass of bitumen, soil and gritty limestone, which can neither be strained or separated by mechanical means. While this tendency or action is serviceable in the preparation of mastic, it renders the rock practically useless for the extraction of bitumen.

The only material suited for the extraction of bitumen is the stone found at the higher altitudes, which, depending on the cementing property of the lime to retain its rigidity, can be heated to a comparatively high temperature without disintegrating. The effect of heat on this stone—say, 300° F.—leads to the bitumen liquefying and exuding from the minute chambers or cavities in the form of a heavy liquid, having a jet black lustre, which on cooling solidifies to the consistency of solid pitch. A higher heat—say 400° F.—converts part of the bitumen into a semi-oil or tar, and at a still higher temperature decomposition begins.

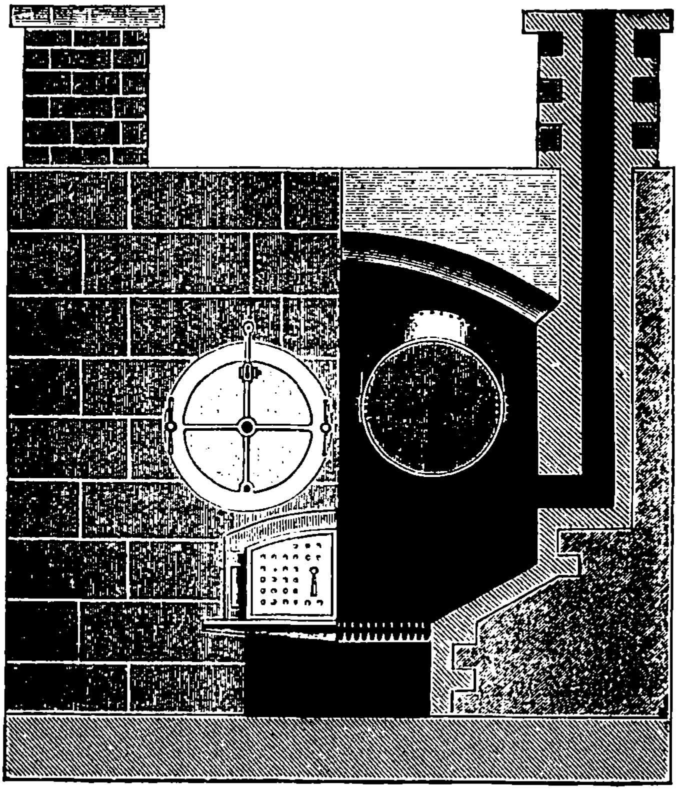

Fig. 1.

Several retorts have been devised for extracting the bitumen from the asphaltic rock and bituminous stone; those for treating the former, it may be stated, have been failures, and need not therefore be described; those retorts, again, which have been devised for treating the hard stone have given more or less satisfaction. The most common form is that shown in Fig. 1 and 1a, which essentially consists of wrought-iron cylinders about 7 feet long by 30in. internal diameter, closed at one end, and placed in an oven with the open end slanting downwards, the angle being just sufficient to allow the bitumen to flow towards the exit formed in the door. Two of these retorts are generally placed in one oven, and several ovens in one bench. The fuel employed is the inferior quality of rock, and the quantity consumed in the furnaces averages one half the quantity treated in the retorts. Each retort contains about 14cwt. of rock, previously broken into pieces the size of a common brick, and the time taken for each charge averages six hours. This form of retort answers fairly well for the bituminous stone, but has failed in extracting bitumen from the asphaltic rock, due to the gritty matters subsiding to the bottom of the retort and forming a barrier to the outward flow of the liquid, which at the same time prevents the heat from penetrating to the interior, a higher heat to effect this causing the formation of a scale, which rapidly leads to the destruction of the retort.

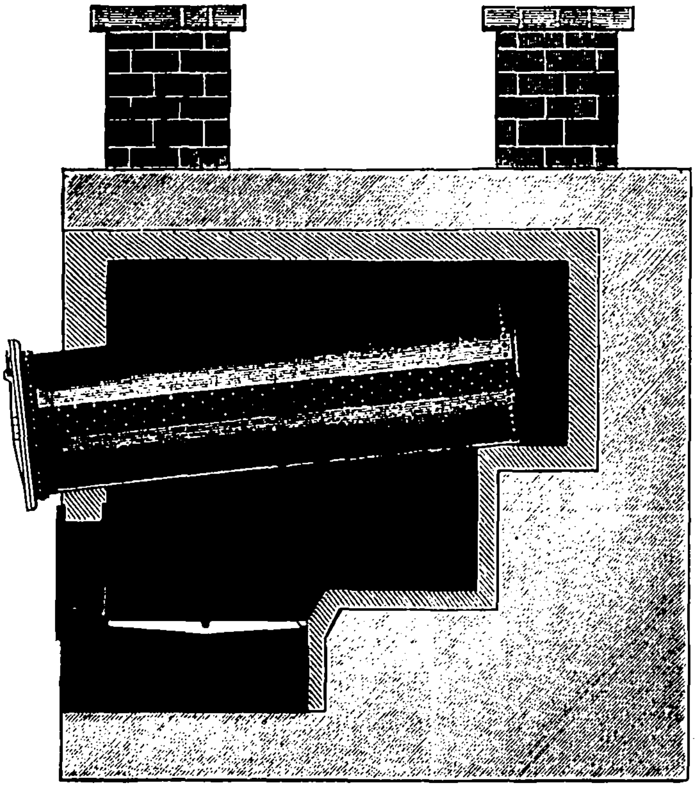

FIG 1A.

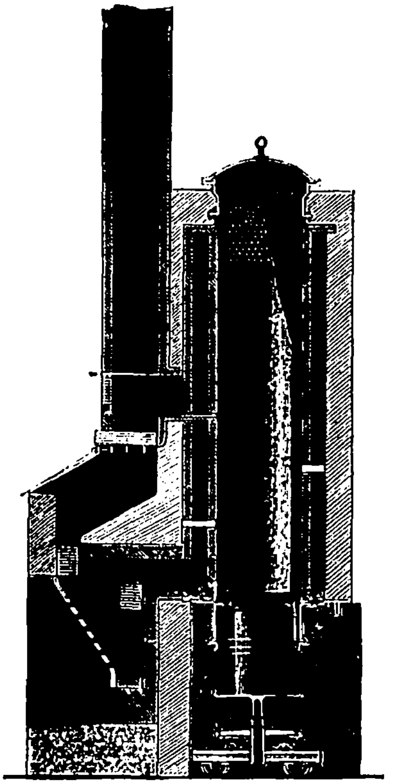

FIG. 2.

A better designed retort is that of Mons. Boutigny, shown in Fig. 2, which was awarded a silver medal at the Paris Exhibition. The retort is built in an oven, vertically, its main characteristics being the *arrangement for preserving the interior of the retort at a uniform temperature*; this is accomplished by having an inner vessel, resembling the shell of the retort, but small enough to fit inside, a space being left between the outside of the inner vessel and the inside of the outer shell, to permit a circulation of vapours in the annular space.

The inner vessel contains the rock to be treated, and having its sides perforated, allows any vapours generated to pass into the annular space referred to, and circulating between the two walls, tends to prevent unequal heating. The bitumen is collected at the bottom by a receiver resting on a trolly, and the spent rock subsequently tipped into a small wagon and removed to the furnace by an inclined railway, sufficient carbonaceous matter remaining in the rock to admit of its utilisation in heating the retorts. The average working temperature is 350° F., and the time taken to treat one charge about four hours. The bitumen obtained working with this form of retort is more pure and contains a less percentage of water than any other, and the labour in charging and firing is greatly reduced by the mechanical devices employed.

OILS.—In nearly all cases the companies floated to work this mineral have made the extraction of oil their main point, the heavy duty on imported petroleum, and the great annual consumption of oils for lighting purposes, proving a great attraction in a prospectus. Invariably, or nearly so, these reports begin as to the magnitude of the deposit, citing some well-known geological expert as the authority, who, making a flying visit through the valley, has allowed his imagination to outweigh his judgment; further, the ease with which the mineral can be treated, the heavy proportion of beautiful burning oil, and, finally, the *immense profits to be made under the invariable* judicious management.

The number of companies who have actually erected plant to extract oil from the rock are numerous, and the amount of money sunk in the ventures simply incredible, and with but one issue—total failure and loss of the capital.

The operations have usually been commenced by an attempt to distil the oil from the rock direct in horizontal retorts, in the same way as the Scotch shale before the introduction of the verticals. The result has been failure, which will be evident from what has been said about extracting the bitumen, the tendency to form a scale in the retort being greatly intensified, which, no matter how carefully fired, rapidly leads to the destruction of the retort, while the quantity of oil obtained after one or two charges dwindled down to about one gallon per ton. The next process has been to extract the bitumen, and subsequently distil it in oil stills, which also has proved a costly process, the bitumen containing a large proportion of water and foreign matter, rendering its distillation by ordinary means a tedious operation and a failure financially; the intermediate process adopted in this country by settling the bitumen and allowing the heavier bodies to subside, being inapplicable, owing to the ebullition of the bitumen at the temperature necessary to ensure precipitation.

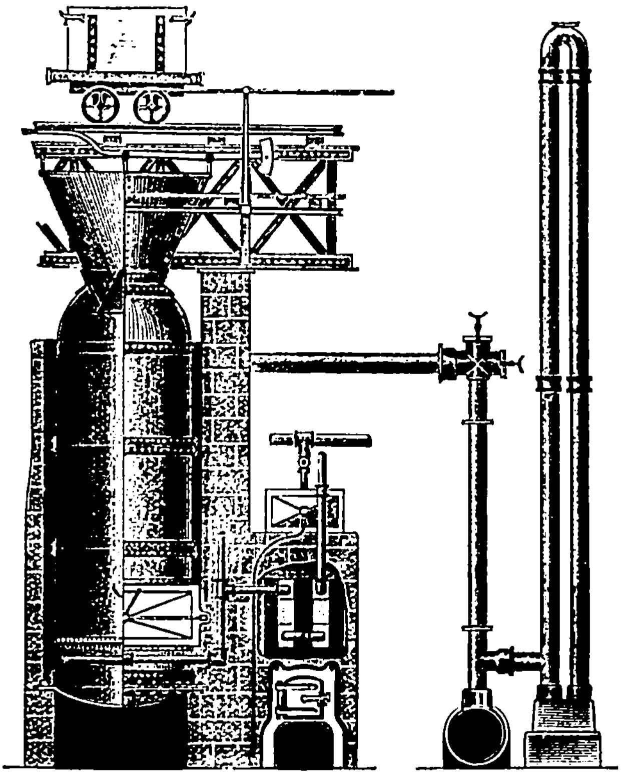

Fig. 3.

The first attempt made to treat the mineral by means of a vertical retort, with the view of extracting oil direct, was initiated by the late Mr. George Bennie, the special retort designed to treat the mineral consisting of an arrangement analogous to the "Boutigny" bitumen retort, the inner vessel being replaced by vertical bars in the form of a wicker basket. The vapours were led off from the top of the retort and condensed in a horizontal condenser. Four retorts were placed in one oven, and the time taken to distil one charge of rock averaged eight hours. This retort, although well adapted for some qualities of the minerals, did not answer for the asphaltic rock, the bars after several months' operations requiring renewal.

Several other forms of retorts were introduced, which, after trial, were discarded; there is no feature of novelty in their design which merits description.

The only process attempted with any prospect of ultimate success was that initiated by the late Mr. William MacIvor—long connected with Young's Company at Bathgate—which consisted in distilling the oil from the rock direct by means of superheated steam applied internally.

Mr. MacIvor, who had been appointed by a local company, some ten years previously in existence, to examine into their mineral and report on the best means of extracting marketable products therefrom, after making a number of experiments in extracting oil and bitumen, decided on trying the effect of passing a heated gas through a charge of rock in a small vertical retort, the gas being maintained at the temperature necessary to ensure distillation. The result showed that by employing steam superheated to a temperature of about 600° Fahr., a yield of about 15 gallons per ton could be obtained in about ten hours' time; the retort containing about 14cwt. of mineral. On this basis plant was erected to treat about 200 tons of rock per week, consisting of six vertical retorts having a capacity of about three tons each. The general arrangement is shown in Fig. 3. Each retort had a superheater—Bennie's 2in. size—and the steam was generated in a steam boiler 14ft. long by 7ft. 6in. in diameter, multitubular type. The condenser consisted of the old style of verticals. Mr. MacIvor unfortunately did not live to see his process carried into practice, and on this account it may not have received justice. Working the full bench of retorts, the yield did not exceed five gallons per ton, further workings showing that a fixed volume of superheated steam, at a temperature of about 600° Fahr., was necessary to ensure distillation of the oil. With the boiler working at its full power, the best result obtained was 12 gallons per ton, with three retorts at work, and the time taken for each charge, twelve hours. The process so far was a total failure financially. The rock to begin with cost about 10s. per ton for mining and transport, and fuel over 20s. per ton for each ton of mineral treated, thus saddling the crude oil with a first charge of over 2s. 6d. per gallon, the market value of which was under 1s. per gallon. The process, while capable of great modifications, possessed several features which greatly added to the cost of operations, only a very small proportion of the heat in the steam was utilised, the greater proportion escaping in the form of latent heat, which again required a great volume of water to ensure condensation. Loss also occurred in the retort through having to raise the temperature of the rock to distilling point before any oil vapour could escape, the action in the retort consisting of a series of condensations and distillations.

At this stage an attempt was made to simplify the condensing arrangement by simply lowering the temperature of the steam issuing from the outlet pipe sufficiently to precipitate or condense the oil vapour without condensing the steam; close observation showed that the steam, at a temperature of about 300° F., contained only an infinitesimal quantity of oil vapour, clearly proving that the mineral contained no light oils. A second modification was made by enclosing the retort in a jacket formed of bricks, the waste gases from the superheaters winding round the retort before entering the flues. This alteration had the effect of reducing the time of distillation to about eight hours, the full percentage of oil in the rock being obtained, the latter presenting the appearance of having been bleached, and without any bituminous taint. The volume of steam necessary was also, in consequence, reduced about one-third, and the purity of the crude oil superior to that hitherto obtained. Further modifications were brought to a termination through the discovery, at this time, that the company who were carrying on the operations had no more asphaltic rock in their mines, the extent of which was supposed to be so great as to induce the company in question to expend from £50,000 to £80,000 in preparing to begin operations.

This led to a more close examination of the formation of the deposit, the result of which is embodied in the views given further on. The bituminous-stone in the higher grounds was tested with the result that two benches of retorts were erected to extract the bitumen, which was subsequently distilled in the ordinary oil stills, the operation being greatly facilitated by adding a proportion of light oil to the bitumen in the still; this had the effect of cutting up or dissolving the semi-solid mass very rapidly when heated, and thereby allowed the water vapour to ascend freely to the surface, and escape without causing frothing or boiling over.

The oil obtained from the distillation of the bitumen differs greatly in its properties from any other known oil, its most marked characteristics being its unusual density, its freedom from ammonia, scale and spirit, or light oils.

One thousand parts of bitumen distilled and rectified

Yielded.	Sp. gr.	Per cent.	Flash-point.
Burning Oil	·850°	150	130°F.
Intermediate	·915	330	250
Lubricating	·990	105	315

The oil obtained from the rock direct, by the superheated steam process, had a density of about ·985, 1000 parts of which yielded when distilled and rectified—

Oil under sp. gr.	·900	50 parts.
Oil sp. gr.	·915	150 „
Intermediate sp. gr.	·915	300 „
Lubricating sp. gr.	·995	155 „

There are no bye-products. An attempt was made, with fair success, to convert the spent rock into lime, the plan adopted consisting in storing the rock when just drawn from the retort in a mound, and allowed to burn. The residue consisted of a very pure lime, but tainted with a bituminous odour.

So far as the manufacture of oil is concerned, the duty on imported petroleum will never influence the production of burning oil from this asphaltic rock. The density is too great to enable its being employed in petroleum lamps; the heavy percentage of sulphur compounds present also materially adds to the cost of refining, while its complete removal is hardly possible even with careful treatment. The only use to which it can be applied is in street lighting or lighthouse purposes, where special burners can be introduced for supplying the necessary quantity of air to ensure construction. Its high flash-point is one recommendation which is more than counterbalanced by the preponderating disadvantages. The intermediate oil is only adapted for cleaning and mixing purposes, its body being equal to the Scotch ·803 intermediate.

The lubricating oil makes a splendid lubricant when mixed with a percentage of vegetable or fatty oils; its high density is not an index of its viscosity, the final distillate has a body several times greater than the finest shale oil, but it loses this property very rapidly on being heated, at least in a much greater ratio than in any other known lubricant. It thus demolishes the old saying met with in treatises and pamphlets on oils, that the density of a mineral oil is a safe criterion of its viscosity.

Origin.—It becomes an interesting point to deduce from the formation of this mineral data to enable some notion being formed of its probable origin, and the bearing it has on the supposed oil belt theory.

It is self-evident that a relation exists between the deposits of bituminous rock which have been described under the term of "asphaltic rock," and the harder material under "bituminous stone." The lower deposit, I believe, has simply been formed through the overflow of oil through a higher level; its association with limestone can be accounted for when we consider the effect of an acid in solution on calcium carbonate; the fact of clay forming a break in the downward descent of the oil, likewise shows that the solution has been diverted or prevented from penetrating through non-absorbing material.

The deposit also being superficial and in general mixed with broken limestone and boulders, demonstrates that the latter have found their way from a higher level at a period antecedent to the oil overflow. Then if the stone found at the higher altitudes be closely examined, some other way must account for its formation. The presence of quartz, and the lime being the binding agent, would show that the bitumen has been deposited or secreted in the minute cavities by some other agency than downward percolation. I am inclined to account for its origin by assuming that the vein at one time formed a natural depression in the mountain side, which had become partially silted up afterwards through some subterranean agency; the mountain was subjected to heat, this heat acted on a bed of lignite, other carbonaceous material causing a distillation, the vapours in ascending found vent in this depression, the heavier vapours partially condensing, and the more volatile vapours escaping. It is possible, of course, that the heat acted on a reservior of oil, perhaps the vein or belt of oil now believed to exist or pass through this identical spot; but to strengthen the supposition that it was a solid carbonaceous body, a layer or bed of liginite crops out of the mountain about twenty miles distant, and it is of course possible to have been a continuation of the same strata. Even with oil, there seems to me to be no more rational way of accounting for its presence than by assuming that a vast bed of carbonaceous material imbedded in the earth was subjected to heat, which causing contraction, would leave a space for the storage of oil and vapour, the rigidity of the earth at the depth preventing subsidence.

In any case, unless the distilling action is still going on, the oil must, through long ages, have become reduced or exhausted; only a very small proportion could have been caught by the surface soil, the greater volume would find its way into the rivers, and thence to the Adriatic. A close examination of the river banks confirmed this view, and showed also that the period of the overflow was comparatively limited, the indications being confined to one line or strip in the sides of the channel. To still further confirm this supposition, Cicero, in one of his histories, mentions, as a wonder or curiosity of the age, that the river Aternus flowed with blood. The Aternus here spoken of is the same river which flows through the valley to the Adriatic, known now as the river Pescara, and it is very probable that what was supposed to be blood, was really the oil on its way to the sea.

There is evidence in the local archives that oil was sent from the district to Rome, Venice, Turin and Milan for several centuries back. The oil being utilised for street lighting purposes, and the bitumen as a cement for water cisterns. But whether the oil was then distilled from the bitumen or consisted of the natural tar which exuded from the mountain side after heavy rains, is a point I failed to find out.

The indications of oil throughout the country bear a strong resemblance to each other in the low proportion of burning oil and the similarity in properties.

No stronger indications exist in the country than those at the opposite angle of the depression, or basin, about seven miles distant, near a village named Socco Casauria. At the present day a heavy black oil—sp. gr. ·950—exudes from the mountain-sides after heavy rains, corresponding in its properties to the oil obtained from the asphaltic rock. Several attempts have been made to strike oil at this spot and in its vicinity, a great impetus having been given to speculation in this direction after the American oil wells were established. In 1863, an Italian gentleman, Maurizio Laschi by name, began operations on a small scale, and ultimately formed a French-Italian company, who erected a refinery and made four trial borings at two different points in the mountain-side, close to the outflow of oil. One well was sunk 1550 feet, a second 540 feet, and the third and fourth about 900 feet each. Oil was obtained at different levels, but in comparatively small quantity, and insufficient to cover working expenses. The operations were conducted by a company of Canadian borers under the direction of an Italian engineer, named Euphemio Ribighini. The altidude of the ground where the borings were made was about 1520 feet above sea level, so that they penetrated about 30 feet beneath the sea line. The following is the result of one distillation of the oil:—'

1000 parts sp. gr. ·951;
Oil = 635
Water = 29
Residuum = 322
Gas = 11

The once run oil on being redistilled and refined gave the following fractionations:—

Boiling Point.	Proportion.	Sp. Gr. 60° F.	F.P.	Notes.
Under 350° F.	·0053	·78	900°	Clear.
,, 400	·0079	·79	123	—
,, 410	·0101	·80	150	—
,, 430	·0062	·81	177	—
,, 445	·0078	·815	183	Pale Yellow.
,, 465	·0047	·82	203	—
,, 480	·0072	·83	211	—
,, 500	·0063	·84	223	Yellow.
,, 520	·0062	·85	246	—
,, 570	·0098	·865	273	—
Over 570	·0098	·875	281	Grease.
,, 570	·0053	·890	320	—
Residuum.	·0088	—	—	Solid.

The result of this spirited attempt to tap the supposed oil belt was a total failure; the pumps were kept at work for about 18 months, each being wrought in rotation to allow the remainder to recuperate; even then the quantity of oil obtained barely covered cost of fuel and attendants' wages.

The oil was refined and employed in specially constructed lamps for street lighting, the sulphur compounds having been previously partially removed by a treatment with acetate of lead, and subsequent prolonged heating in presence of caustic soda. Oil has also been found at other points in the country, particularly in Veghero, near Rivanazzano. The oil corresponds in its properties to that already mentioned, the density being a shade less, but the odour much more disagreeable, due to the presence of sulphuretted hydrogen. The density of the oil at 60° F. is ·915, and the result of treating 1000 parts is as follows:—

Boiling Point.	Proportion.	Density.	F. P.	Notes.
220° F.	100	·875	65° F.	Strong Odour.
230	70	·88	75	Clear.
240	99	·89	85	,,
250	93	·895	98	,,
260	107	·90	108	,,
270	81	·92	114	Yellow Tinge.
280	118	·925	117	Yellow.
310	121	·93	145	,,
Above 310	92	·945	156	Dark Yellow.
Do.	53	·95	175	Grease.
Residuum.	61	·974	200	Solid.

In concluding, it seems to me that further attempts at boring for oil will prove both fruitless and unremunerative. Oil may have existed in quantity at one time; but all the evidence points to the fact that it has been dissipated or dispersed, and it is the remains of the past extensive distillations which is now scattered over the country.

DISCUSSION.

Mr. Henderson: I understand that Mr. Murrie attributes the formation of these bituminous deposits to what is really a case of natural destructive distillation of a bed of lignite. Have any traces of such a bed been discovered, or do the rocks in the neighbourhood of the deposit appear to have been subjected to such heating or metamorphism as would be necessary to produce this alteration in the lignite?

Mr. Murrie: There is no superficial evidence of the rocks having been subjected to heat at a period corresponding to the time of the supposed oil distillation or overflow. A packet of calcined clay was found in one of the veins of bituminous stone at an altitude of 2800 feet; but its presence could be accounted for in another way. Had it been possible to excavate the vein of bituminous stone to a sufficient depth, I believe evidence would have been obtained to determine with certainty the cause of the bituminous deposit. The bed of lignite, which cropped out in the same range, but about 20 miles distant, probably passed through the district; but the accumulation of *débris* from higher ground hid from view indications of this and other strata, which would, perhaps, have aided in arriving at more definite conclusions. I understand Sigr. Laschi, already referred to, started a factory in the vicinity of Civita-Vecchia, with the view of extracting oils from the lignite found in that district; but I do not know with what results. The oil obtained from the beds of lignite on the Mediterranean side is too dense to be of value in competing with petroleum; it is used for gas-making purposes, and mixing with American petroleum, for street lighting, where special lamps are employed. The more dense oil is refined and mixed with animal and vegetable oils for lubricating purposes, but on a very small scale.

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DESCRIPTION OF A SERIES OF NEW OR IMPROVED THERMOMETERS FOR TAKING HIGH TEMPERATURES.

BY MR. JAMES MURRIE.

At the January meeting, time did not permit my explaining more fully the principles embodied in these new temperature indicators, and the various details of their construction.

The metal mercury, for well-known reasons, has been universally adopted as the best means of obtaining indications of variations of temperature, within certain limits—viz., the freezing and boiling points of the liquid. The freezing point does not require to be considered in this case; and the boiling point under atmospheric pressure is allowed to be 662° F., and near 600° in a vacuum. In the construction of an instrument such as the common mercurial thermometer, it is invariably the custom to exclude atmospheric air from the inner part of the instrument, the space reserved in the stem for the expansion of the mercury being a vacuum, or nearly so. The upward limit of the range of the indicator is thus between 600 and 662° F., depending on the length of the stem, and one or two minor conditions. It is evident that the higher boiling point of the mercury under atmospheric pressure is due to the increased resistance to ebullition; and, reasoning from this, it is evident that by still further increasing the resistance, a greater range of thermometric readings can be obtained. On this principle depends the action of the more important of these heat indicators, virtually being an extension of the ordinary mercurial thermometer.

The most simple means of increasing the resistance to ebullition is obviously the utilisation of the great density of the liquid, in having a vertical column sufficiently long to give the desired pressure on the mercury in the bulb. Another, and perhaps not more difficult way, while it has the advantage of dispensing with the long stem, is in having an elastic fluid, such as nitrogen, in the confined space above the mercury, either in the form of a bulb or elongated chamber, the quantity of the fluid being just sufficient to resist the expansive action of the mercury, and thereby generate an opposite pressure, which is the direct means of raising the boiling point. The glass would, of course, not be adapted for comparatively high temperatures, although suited, if the thermometer be carefully constructed, to indicate temperatures to 750° F. The instrument I have had in use for nearly two months, indicated to 728°, and so far shows no sign of alteration.

For higher temperatures glass would either have to be discarded or some means taken to preserve the bulb from becoming enlarged or bulging outwards under pressure. Fig. 1 shows one means whereby such an arrangement is adopted, and a glass thermometer, made to indicate temperatures to 850° F. It consists in enveloping the thermometer proper in an outer jacket, formed of two tubes connected near the centre. The bottom tube may be of iron to withstand the action of the heat, and the top tube of glass to enable the movements of the mercury in the inner stem being seen. The annular space is filled

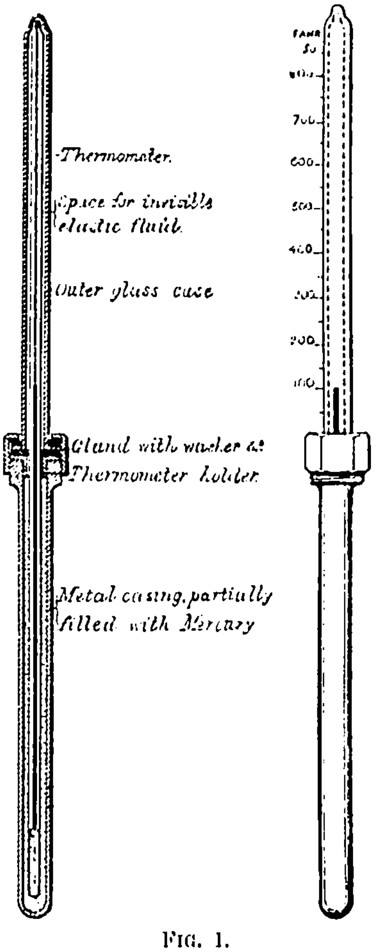
Thermometer.
Space for invisible elastic fluid.
Outer glass case
Gland with washer as Thermometer holder.
Metal casing, partially filled with Mercury
FAHR
800
700
600
500
400
300
200
100

FIG. 1.

with mercury in the bottom part, and a neutral elastic fluid in the top of the chamber, which is in communication with the thermometer stem. The mercury in the annular space on being heated expands, compresses the elastic fluid in the top part, and at the same time creates an internal pressure equal to and corresponding to the pressure tending to cause distortion, or in other words the outward and inward pressures being equal there is no tendency to alter the form of the central thermometer. The glass is maintained in position by a fluted washer, made to embrace the body of the stem. This plan may be applied to the ordinary thermometers, thereby removing the complaint about the zero point not remaining constant, when the instrument was in constant contact with the heated body.

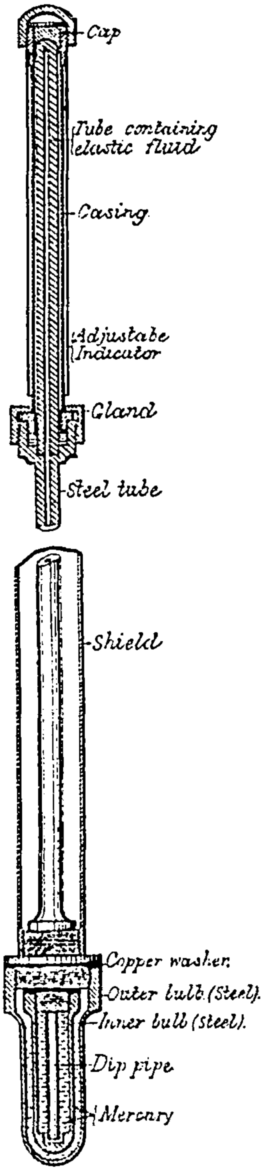

FIG. 2.

For temperatures above 850° F., it is preferable having the bulb made of iron or steel, as shown in Fig. 2, designed to indicate temperatures up to 1000° F. This instrument is but a modification of the one described at the January meeting, the elastic fluid in this case being nitrogen. The indicator tube containing the elastic fluid has a fixed relation to the capacity of the bulb, the aim being to generate sufficient pressure to prevent ebullition without unnecessary strain on the apparatus. By arranging the bore of the tube to contain the necessary volume, and having a clear daylight of 11in., each inch exactly corresponds to a range of 100° F., the temperature at 1000° F. corresponding to 10in. on the glass. The bulb is protected from distortion by an arrangement analogous to the more delicate instrument shown on Fig. 1. In this case the bulb alone is jacketed, the annular space being partially filled with mercury, the saturated vapour of which gives the pressure corresponding to that in the minor bulb. The inner or bulb proper is thus free from all tendency to distortion. This form of instrument has been most

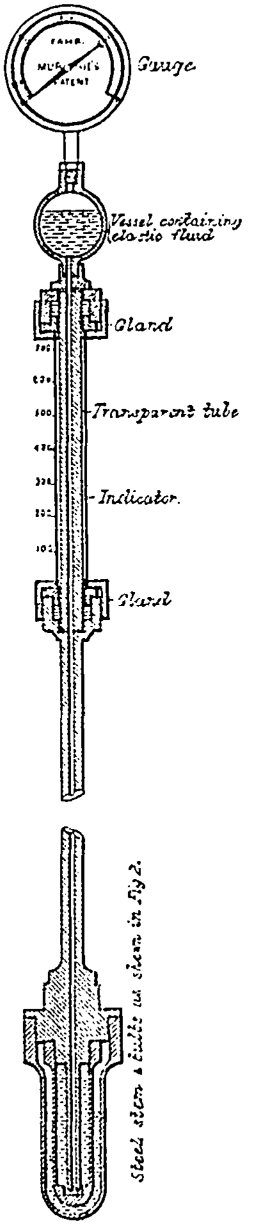
FAHR.
Gauge
Vessel containing elastic fluid
Gland
Transparent tube
Indicator.
Gland
Steel stem & bulbs as shown in Fig 2.

FIG. 3.

severely tested to ascertain the effect of heat and mercury combined on the bulb, and so far has given satisfaction. There are a variety of modifications of this form which do not require special notice, some of which will at once occur to you, such as employing the saturated vapour of ammonia or carbonic acid in the upper chamber. The foregoing apparatus are mainly intended to take indications of temperature with comparative rapidity. The mercury only having to absorb a specific quantity of heat, no time is taken up in generating vapour.

Where the instrument is a fixture, as in the case of an oil still, a rapid indication is of less importance, the temperature of the gases in the vessel taking some time to vary, allows the apparatus time to accommodate itself to the new conditions. In such cases the apparatus may be constructed in a slightly different way. The mercury may be maintained in a liquid condition by the pressure of its own vapour, generated in and confined to the bulb, this pressure being measured by a pressure gauge. The arrangement is really based on the same principle as Schaeffer's Tension Thermometer, only the means of carrying out the principle differ very materially. Here it is the bulb alone which is heated, or brought in contact with the heated body. Any vapour generated cannot escape from the bulb, the pressure of which is communicated to the gauge by a column filled with mercury. This arrangement is shown in Fig. 5.

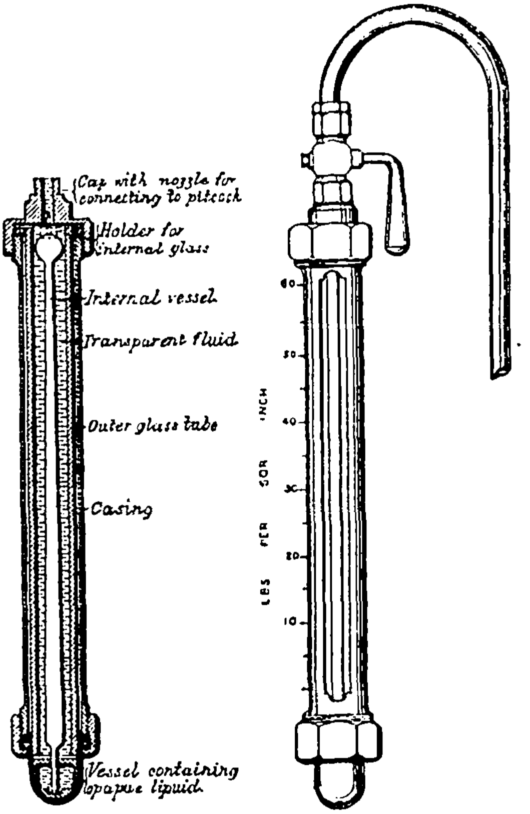
Cap with nozzle for connecting to pitcock
Holder for internal glass
Internal vessel
Transparent fluid
Outer glass tube
Casing
Vessel containing opaque liquid
LBS PER SQR INCH
60
50
40
30
20
10

FIG. 4.

Fig. 3, it may be added, is a combination of the two principles, the expansion of the mercury being shown by a transparent glass tube forming a continuation of the stem, and the pressure of the saturated vapour measured by a Bourdon or own form of gauge. In determining the pressures of the different vapours, it became evident that some other gauge than the Bourdon would have to be adopted if accurate indications were to be obtained. The spring of the gauge when subjected to a comparatively low heat, either lost its elasticity or the expansion of the spring was magnified through the rack and pinion arrangement. For indicating pressures in steam boilers, the Bourdon may be fairly accurate, but it is not adapted for laboratory work, where extreme accuracy is essential. The sketch (Fig. 4) shows a pressure gauge designed as an adjunct to the thermometers, and which promises to compete with any metal spring gauge, having the advantage in cost, accuracy and sensitiveness. It consists of a glass tube sealed at one end, and having its terminal widely separated by the device of employing a bulb on the extremity of the tube. This tube is filled with an elastic fluid, and its open end dipping into a vessel filled with an opaque liquid. Surrounding this central vessel is an outer chamber, and the annular space filled with a transparent liquid. The instrument on being connected to the generator, and the valve opened has its contents subjected to hydrostatic pressure, which compresses the elastic fluid in the central vessel to the extent shown by the ascent of the opaque liquid, the pressure existing in the generator being shown by an indicator

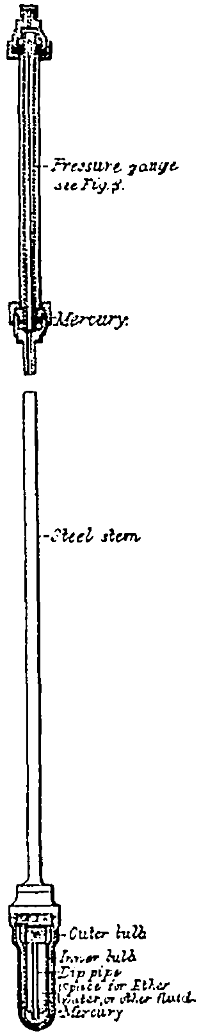
Pressure gauge see Fig. 4.
Mercury
Steel stem
Outer bulb
Inner bulb
Dip pipe
Space for Ether water, or other fluid.
Mercury

FIG. 5.

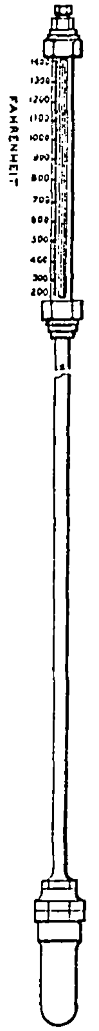

FIG. 6.

fixed on the outer casing of the gauge. In practice the instrument has answered well for high pressures, and it has been and is being fitted on steam boilers with satisfactory results.

Figs. 5 and 6 illustrate this form of instrument, with one of these pressure-gauges attached. The bulb can be filled with any fluid which will not act on the mercury or metal forming the bulb, and the pressure of the saturated vapour measured by any known gauge, due allowance being made for the pressure caused by the column of mercury in the stem. Working with mercury alone, temperatures ranging to 1400° F. can be indicated.

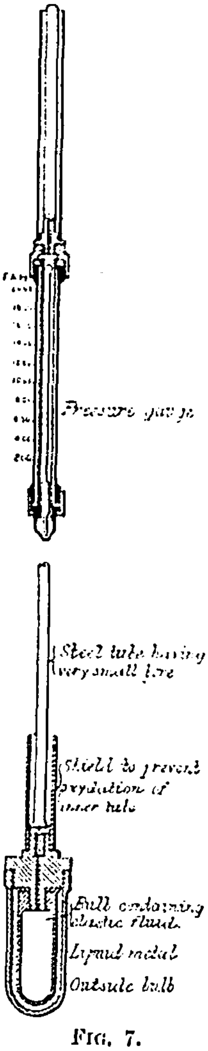
Pressure gauge
Steel tube having very small bore
Shield to prevent oxydation of inner tube
Bulb containing elastic fluid
Liquid metal
Outside bulb

FIG. 7.

Another form of apparatus, specially designed for temperatures approaching 2500° F., is that shown in Fig. 7, which is practically a gas thermometer, but possessing one or two features of its own. The gas is maintained at a constant volume in a steel casing or bulb of high melting point, and the pressure measured by a very sensitive gauge of the foregoing design. This bulb is enveloped in an outer casing, the annular space being filled with a fluid which, while preserving the form of the bulb proper, at the same time is made to prevent diffusion of the gas in the central vessel by having an equal or greater pressure than that existing internally. The outer jacket may be surrounded by, or enveloped in, some material which prevents oxidation, without retarding the motion of the heat. For temperatures under 2000° F. it has answered fairly well in the preliminary trials, and experiments are being made to test its efficiency at still higher temperatures.

These are the most striking forms of these new or improved heat-indicators. To give a description of each design would try your patience, considering that there are over thirty-six forms of the apparatus, the greater number of which are for indicating temperatures up to the softening point of glass. The pressure-gauge, also, can be constructed in twelve different forms, depending on the pressure it is desired to register.

DISCUSSION.

Professor MILLS said: I would like to ask Mr. Murrie a question about the thermometer on figure (No. 3) showing the lowest range of temperature. It is well known that in constructing a thermometer the operator heats the stem very strongly, and afterwards drives the mercury up it; the result of it is that where the mercury comes in contact with the hot stem it drives all air before it. At the extreme range of the mercury the thermometer can thus be sealed free from air. The reason for this is, of course, that if we have air inside the thermometer, and allow the mercury to move up and down, the air gets between the mercury and the glass, in varying amounts according to temperature. The same objection should apply to nitrogen or other gas. I should like to ask Mr. Murrie if any of these irregularities have been observed, and if it has not been found necessary to get rid of them by some means or the other. Then, again, as regards the sketch on my extreme left (Fig. 6), the bulb of the thermometer is filled with mercury, and is kept from deformation by being surrounded by mercury in another bulb. I suppose for this very high temperature the glass is very thick. The question is, Does the thermometer keep a constant zero, and does the bulb undergo any deformation? Of course the bulb when very strongly heated must become rather soft; and even at lower temperature glass is in fact a plastic substance. At a very high temperature it must become soft, and if thick must tend to float, and ultimately, therefore, become deformed. Of course this is only speculation based upon experience, but I should like to know what happens in both the cases I have mentioned.

In answer to Professor Mills, Mr. MURRIE said: In the glass thermometer indicating to about 800° Fah. a difficulty is experienced in driving out the common air, and replacing it with nitrogen, a trace of oxygen having the effect of forming small specks on the inside of the tube.

Professor MILLS: But does it not get between the mercury and the glass?

Mr. MURRIE: Not so far as I have been able to determine. If the stem of the thermometer be heated at the same time, in the making of the instrument, no gas will adhere to the sides of the tube, and in practice the gas has not found its way between the mercury and the glass. If it did, the effect would be to break up the column of mercury into one or more sections, while my experience is that the employment of an elastic fluid in the upper extremity of the tube prevents this action, and permits the instrument being lowered in temperature with comparative rapidity, without the fear of the column splitting. Regarding Professor Mills' second question on the liability of the metal bulb changing its shape under continued heating: This can hardly take place if fitted with an outer jacket, and working in the way already described. When the instrument was a fixture and subjected to heat for any length of time, I would depend more on the form of instrument shown on Fig. 3, where the pressure was measured by a gauge, and the corresponding temperature read off. I have made many experiments with a thermometer without the outer jacket, but fitted with an adjustable indicator, and after one or two trials with a new instrument, I found it was not necessary varying the indicator, the metal receding to a fixed point. Of course this was only in taking occasional measurements of temperature, where the instrument would only be a few minutes in contact with the heated body. This instrument is shown on Fig. 8. So far as variation is concerned, the only provision to be guarded against is the expansion of the metal forming the walls of the bulb, but this applies to the ordinary glass thermometer as well, and the variation is so small as to be immaterial.

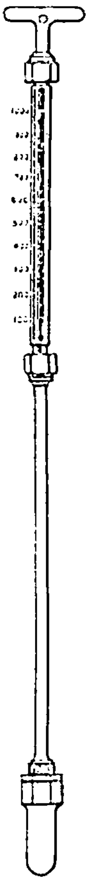

Fig. 8.

Obituary.

Dr. ADRIAN BLAIKIE.

At the early age of 29 years, this promising young chemist passed away on the morning of Saturday, the 7th of February.

Dr. Adrian Blaikie was the fourth son of the Rev. Professor Blaikie, of the New College, Edinburgh. He received his school education in the Edinburgh Academy and Fettes College. When he was eighteen he went to Stuttgart, where he studied chemistry under Professor von Marx, during two years. Returning to Edinburgh, he entered on the science curriculum in the University, where he successively took the degrees of Bachelor and Doctor of Science. His success as a student is shown by his having been awarded the Baxter Physical Science Scholarship, and the Hope Chemistry Prize, at that time the two highest honours attainable in his special department; and his powers as an original investigator were proved by a series of papers on certain organic compounds of sulphur, the joint work of Professor Crum-Brown and himself, published by the Royal Society of Edinburgh, during the years 1877–9. After holding for one year the post of Lecturer on Chemistry at the University College of Bristol, Dr. Blaikie was appointed by the Government to assist the late Dr. R. Angus Smith in his important duties as the General Government Inspector of Alkali Works. To that work Dr. Blaikie brought skill, industry, and enthusiasm; so that he soon became, not only the official assistant, but the trusted friend of his chief. On the death of Dr. Smith, in May of last year, Dr. Blaikie was promoted in the service to the responsible position of the Inspector of the Tyne district; and in August he entered with ardour upon his new duties in Newcastle. But these were not to last long. Dr. Blaikie had never been very strong; and in the last week of November he was seized with an intermittent fever, which, after a lingering illness, resulted in his death early on Saturday morning, 7th February, at his father's house in Edinburgh. This event has deprived many of a friend, endeared to them by his refined and winning nature, and has cut off a young life of great promise.

Journal and Patent Literature.

I.—GENERAL PLANT, APPARATUS, AND MACHINERY.

Improvements in Apparatus for Purifying and Softening Water for Steam Boilers, Laundry and Other Purposes. H. J. A. Bowers, Acton. Eng. Pat. 481, Jan. 3, 1884.

The inventor separates the mineral and other impurities from the feed-water before its entry into the boilers, by first conveying it into a large pipe fixed in the steam space of a boiler. This pipe presenting a considerable surface to the steam in the boiler, the temperature of the water passing through approximates closely to that of the steam; the mineral and other impurities, which are liable to form scale or deposit in boilers, are thus rendered insoluble. The feed-water now passes from the pipe into an apparatus called the separator. This consists of two vertical chambers standing upon, and connected with, a lower vessel, which tapers downwards to a small outlet provided with a cock or valve. The feed-water enters at the top of the smaller vertical chamber, passes from thence into the lower vessel, in which the heavier insoluble particles are deposited, and then finds its way into the larger vertical chamber, entering at the lower end. This chamber contains suitable filtering materials, through which the water finally passes in its upward course, previously to its delivery into the boiler. The heavier impurities are drawn off from the lower tapering vessel by means of the blow-off cock or valve.—E. G. C.